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THE INFLUENCE OF AREA, POSITION, AND BRIGHTNESS IN THE VISUAL PERCEPTION OF A REVERSIBLE CONFIGURATION

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By no means a new problem, but certainly one that has acquired new emphasis in recent years, is the determination of the structuration of perceptual experience. The present paper deals with the influence of three factors, area, position, and brightness, in the visual perception of a reversible configuration. The form employed was a circle divided into six sectors, each alternate sector subtending the same angle at the centre. The form could be seen in two ways according to which set of alternate sectors was perceived as figure (see Fig. 1).

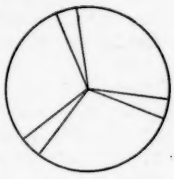

FIG. 1. THE CONFIGURATION EMPLOYED

In terms of this specific form the study here reported attempts to answer the following questions: (1) To what extent does the area of the sectors (or the distance separating their boundary lines) determine which set of sectors will be seen as figure? (2) To what extent are the results of (1) influenced by varying the position of the sectors within the circle? (3) To what extent are the results of (1) influenced by introducing a marked brightness difference in the alternate sectors?

HISTORICAL REVIEW

Doubtless the present problem could easily be characterized as a study in Gestalt psychology. The use of simple visual forms to determine the principles involved in the organization of perceptual experience has, however, had a history antedating the rise of the Gestalt movement. Hempstead, for instance, in 1900 published a study

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in which she made use of simple forms tachistoscopically exposed in order to investigate the perception of visual form. She concluded: "In looking at forms liminally different from their background we are likely to continue lines and to complete figures under the guidance of the two principles of symmetry and similarity."[1] The resemblance of certain of Hempstead's phrases to the Gestalt concepts of "figure-ground," "Prägnanz," "closure," and "law of similarity" is not without interest to the historian. It remains true, nevertheless, that prior to the Gestalt movement there was a distinct tendency to account for the organization of experience by means of some special act. Thus we have the "creative synthesis" of Wundt, the "determining tendencies" of the Würzburg school, the "producing process" of Benussi and other members of the Graz school. Mach and Von Ehrenfels struck a note of originality when they denied the necessity of an act to account for organization and spoke of form as itself being an independent sensory element (the *Gestaltqualität*).[2] From this point on to the emergence of *Gestalttheorie* there was an increasing tendency to discountenance the part played by "attention," "meaning," etc., and to investigate in their stead the objective conditions in the stimulus-object that might determine the perception of form. Köhler, however, has recognized that "the fact in itself that the change of subjective attitude towards the field can to a certain degree alter its properties, units, etc., must be regarded as being a very interesting problem. As yet it has not been studied thoroughly enough."[3]

Among the Gestalt psychologists Wertheimer has been the most prolific in the enunciation of formal principles; nor is this surprising in view of his stress on "inner necessity." In his now famous 1912 article,[4] he was already investigating changes in the experience of movement resulting from variation in the objective conditions of the stimulus-object. In the so-called Gestalt "declaration of independence," he trenchantly expresses the theoretical supposition underlying his investigations of the "laws" of the Gestalt; our experiences are determined, he writes "bei dem Erfassen eines inneren Zusammenhangs, dem Spüren einer inneren Notwendigkeit . . . wo das Geschehende nicht durch blinde *äussere Faktoren* sich bestimmt, sondern durch sachliche *'innere Gefordertheit.'* "[5]

In 1923 followed an article in which the principal determining conditions (the forms of "inner necessity") were listed together with numerous illustrative figures.[6] Only those principles investigated in this study need be considered here.

In this study the factor that, following Graham[7], has been termed "area" (as being more appropriate to the figure employed) is called by Wertheimer the "factor of nearness." "Die Zusammengefasstheit resultiert—ceteris paribus—*im Sinn des kleinen Abstandes (Faktor der Nähe).*"[8]

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[1] L. Hempstead, The perception of visual form, this JOURNAL, 12, 1900, 192.
[2] Cf. I. M. Bentley, Psychology of mental arrangement, *ibid.*, 13, 1902, 269-293.
[3] W. Köhler, An aspect of Gestalt Psychology, *Psychologies of 1925*, 1926, 177.
[4] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 51, 1912, 161.
[5] Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1, 1922, 57.
[6] Wertheimer, *idem, ibid.*, 4, 1923, 300-350.
[7] C. H. Graham, Area, color, and brightness difference in a reversible configuration, *J. Gen. Psychol.*, 2, 1929, 470-481.
[8] Wertheimer, *op. cit.*, 1923, 308.

According to this principle, when we have forms in which all other factors are held constant, the figure that will be perceived will be that which is formed by the parts in closest proximity. Thus in Fig. 2 one usually perceives ab/cd/ef, and not a/bc/de/f. Although such an example may appear almost ludicrously simple, it must not, says Wertheimer, be passed over with a "smug assertion of self-evidence." A reference to Wertheimer's article will convince the most skeptical that there are numerous complicating factors that enter into the demonstration of even such an apparently simple principle as that of "nearness." Although Wertheimer's article may now be taken as the classical exposition of the internal laws of the Gestalt, the argument is largely empirical; no specific experimental results are quoted.

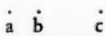

FIG. 2. ILLUSTRATING THE FACTOR OF NEARNESS

The work of Rubin in the field of visual perception,[9] although done for the most part before Gestalt psychology became an articulated movement, has been largely incorporated in Gestalt literature. Rubin's investigation was particularly directed toward an experimental elucidation of the figure-ground phenomenon; with this aspect of his work we are not now concerned. During the course of his experimentation, however, Rubin employed a circle form that is the same as the one used in the present study with the exception that it had eight instead of six sectors. Rubin did not attempt to isolate any of the factors later mentioned by Wertheimer; his general conclusion involves but two factors, area and colour. He says, "One can advance the following, fundamental law: If of two homogeneous, differently coloured fields the one is noticeably larger than the other and surrounds the other, then there is an overwhelming probability that the small, inclosed field is perceived as figure."[10]

Wever has published two articles dealing with the figure-ground phenomenon;[11] like Rubin he was particularly interested in discovering the distinguishing properties of figure and ground. He found that "one of the factors in equivocality is equality of the areas of the two stimulus fields."[12]

Much has been written by Köhler on the principles of the Gestalt, the emphasis being rather on the theory underlying Wertheimer's principles. Although Köhler has apparently worked with the form used in the present study, no specific references to experimental studies could be found. He writes,

"In (the form) we are confronted by a pattern formed by three narrow sectors. Looking at the center of it, most persons will suddenly see another pattern after a while. In the new shape those lines, which belong together in the first pattern as contours of one arm, belong to separate arms and vice versa. The organization is changed. By appropriate experimentation, making our subject fixate the center for a long while, we can reduce the time for either shape to a minimum value, so that the alternations follow each other very quickly. If now the figure is turned around a little, so that the arms have another position, the figure becomes as stable again as it was at first, and only after some time the alternation will quicken, as in the first position."[13]

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[9] E. Rubin, *Visuell wahrgenommene Figuren,* 1921, 79.

[10] *Ibid.,* 79. The translation of the excerpt is taken from Graham (*op. cit.,* 470).

[11] E. G. Wever, Figure and ground in the visual perception of form, this JOURNAL, 38, 1927, 194-226; Attention and clearness in the perception of figure and ground, *ibid.,* 40, 1928, 51-74.

[12] *Op. cit.,* 1928, 55.

[13] Köhler, *Gestalt Psychology,* 1929, 186.

Although Köhler is here more interested in the phenomenon of fluctuation than in the factor of area, the passage is of some importance as references to positional variation of such figures are not abundant.

Graham, using a circle form with six sectors, investigated the influence of area, color, and brightness. His method differed considerably from that employed in the present study. His circles, 15 cm. in diam., pasted on a neutral gray background, were hung on a wall 230 cm. from the *O*. From this distance *O* fixated the center of the circle for 16 periods of 30 sec. each; during fixation *O* pressed a telegraph key when the small sectors were seen as figure and released the key when the large sectors were figure. The results were recorded on a kymograph. In investigating the influence of area, Graham varied the angles subtended by the alternate sectors. Beginning with a circle whose alternate sectors were 10° and 110°, he increased the angle of the small sectors by intervals of 10° until the alternate sectors were equal (each 60°). Brightness was investigated by presenting a series in which the alternate sectors were colored blue and red, the red sectors being the brighter. The influence of color was determined by presenting other series in which the alternate sectors were blue and white, blue and red, red and green. Although Graham makes no statement regarding the position of the sectors when they were presented to his *O*s, the illustration accompanying the article shows one of the small sectors perpendicular to the base of the form. Graham's conclusions were:

"Our results show that in this type of configuration (1) the system of smaller fields is chosen as 'figure,' and a relative decrease in the size of these fields makes them more dominant, *i.e.*, they persist as 'figure' longer in reversals. Dominance varies inversely in a liminal manner with area. When the systems are equal in area, the configuration is most equivocal. (2) When 'figure' and 'ground' are of equal brightness, area is a most important factor in the perception. This seems to be the case, regardless of color differences. (3) When 'figure' and 'ground' are not of equal brightness the influence of area is greatly affected, and, if the brightness differences are great enough, the tendency is for the brighter system to dominate."[34]

Graham's results for the series in which area was the variable were quite consistent for his 3 *O*s. Measuring dominance in terms of the percentage of the total time that a figure was perceived, he found that the smallest sectors (10°) were seen as figure about 73% of the time and the sectors of 60° (all sectors equal) were seen 50% of the time, the time the small sectors were seen as figure decreasing with fairly even gradations from 73% to 50% as the small sectors were increased in size. The results for the brightness series indicated a well-marked dominance of the red bright sectors over the blue less bright sectors. In view of the fact that Graham has here introduced a color difference as well as a brightness difference, and especially since his own results show that when brightness is equated red tends to be more dominant than blue, his results for the brightness series may be open to question. It may also be noted that Graham's results for the brightness variable are only based on the reports of 2 *O*s for only two different forms (30° and 60°).

In summary, it may be seen that among recent investigators there is a fairly definite conviction that the variation of area in the stimulus-object does result in a variation in the perception of figure, with opinion unanimous that the smaller or more closely circumscribed unit (or units) of a configuration is usually seen as

[34] *Op. cit.*, 481.

figure (all other factors, of course, being constant). The influence of brightness does not appear to be quite as definitely known and is more open to question. The influence of positional variations in a reversible configuration of the type used in this study also appears to have received less attention on the part of experimenters than such factors as area, similarity, closure, etc. Although Wertheimer has worked with such variables as area, etc., their range of application is still undetermined; for, although Wertheimer himself has formulated a number of "laws" to express their operation, he does not hesitate to warn his readers that experimentation would have to be performed with a greater diversity of figures to permit the formulation of laws that would have a wider application and significance than can now be claimed for them.

Method and Procedure

Apparatus. The exposure apparatus was a modification of the Dodge tachistoscope.[15] It comprised two wings, one for the prefixation-field and one for the fixation-field. The former was illuminated by 6 and the latter by 2 12-volt miniature bulbs. As the prefixation-field was seen indirectly by means of a mirror arrangement, a larger number of bulbs was required to secure illumination equal to that of the fixation-field. Equality of illumination was only subjectively estimated. The fixation- and prefixation-fields were each supplied with a slot from which a circle 5 cm. in diam. had been cut. The forms were placed behind the slot of the fixation-field and held tightly against it by means of a strong spring; they were seen at a distance of 38 cm. from the *O*'s eyes. The tachistoscope could be raised or lowered to the height most suitable to *O*.

Illumination of the fixation- and prefixation-fields was secured by means of 3 points making contact with the brass drum of a spring kymograph. By means of 2 sets of slots in the insulating paper covering the drum, the 2 fields could be illuminated for the required time, the prefixation-field being darkened synchronously with the lighting of the fixation-field. The third contact point and a third set of slots illuminated a small warning light 2 sec. before the fixation-field came into view. Eight 1.5-volts dry-cell batteries wired in series provided the necessary electrical energy.

Materials. In the investigation of the three factors of area, position, and brightness five different series of the circular form shown in Fig. 1 were used.

Series 1. This comprised a set of 7 circles, each 5 cm. in diam. The variable was the area of the sectors as measured in terms of the angle subtended at the centre. The alternate sectors were equal. The angles of the sectors for each circle were as follows:

Circle	*Small Sector*	*Large Sector*	*Circle*	*Small Sector*	*Large Sector*
1	10°	110°	5	50°	70°
2	20°	100°	6	55°	65°
3	30°	90°	7	60°	60°
4	40°	80°			

It will be noted that small sectors are increased by 10° each time except for the sixth and seventh circles where the increase was reduced to 5°, preliminary experimentation having indicated that finer gradations were required as the sectors approached equal-

[15] R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10, 13.

ity. In Series 1 the circles were so drawn that the lowermost of the small sectors deviated 45° to the left of a perpendicular drawn from the center of the circle to its base (see Fig. 3). For convenience in designation this will hereafter be called the "indifferent position."

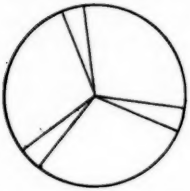

FIG. 3. POSITION OF SECTORS IN SERIES 1, 4, AND 5

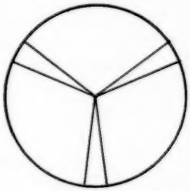

FIG. 4. POSITION OF SECTORS IN SERIES 2

FIG. 5. POSITION OF SECTORS IN SERIES 3

Series 2. This was the same as Series 1 with the exception that the lowermost of the small sectors in each circle was drawn from the center perpendicular to the base of the circle, the large sectors being perpendicular to the top of the circle (see Fig. 4).

Series 3. This series is the complement of Series 2, the small sectors being drawn perpendicularly from the center of the circle to the top of it, the large sectors being perpendicular to the base (see Fig. 5). As the 60° circles for Series 2 and 3 would be identical, this form was omitted from Series 3.

Series 4. This series and the succeeding Series 5 were designed to investigate the influence of a marked brightness difference in the alternate sectors on the perception of the figures. Graham, as we have already seen, investigated brightness by having the alternate sectors blue and red, the red sectors being the brighter. This method, however, introduces, in addition to the brightness difference, a color difference. If color were to be introduced at all it would appear preferable to use two brightness values of the same color. Since, however, Graham found that color exerts some influence in the perception of the figures in the circle form, it appeared preferable to vary brightness in an achromatic rather than in a chromatic scale. Hence in Series 4 black and white were used for the alternate sectors, the small sectors being white and the large sectors black. In all other respects this series is identical with Series 1. It is not to be supposed, however, that the use of black and white is free from difficulties. As Graham, himself, states, "equivocal figures presenting black and white areas would not seem to be good, for the subject's past experience with printed material, etc., would create a set for the black as 'figure'."[16] Black and white were used, nevertheless, for two reasons: (1) if a chromatic scale were used an unknown factor would still be entering into the experiment, whereas the use of black and white would at least enable one to determine whether results for achromatic brightness difference were different from results obtained with a chromatic scale; and (2) it seemed desirable to discover whether a marked brightness difference such as black and white would give results that were the same as, or different from, results obtained from the blue-white form used by Graham.

[16] Graham, *op. cit.*, 478 f.

Series 5. This was the complement of Series 4, the large sectors being white and the small sectors black.

All the forms were drawn on white filing cards. Only the boundary lines demarcating the sectors were drawn, the contour of the circle being formed by the fixation-field diaphragm from which a circle 5 cm. in diameter had been cut. The forms were so drawn that when inserted behind the diaphragm the point where the apices of the sectors met fell exactly at the center of the circular perforation. The lines bounding the sectors were drawn with India ink at a constant width of 0.4 mm. In Series 4 and 5 in which the alternate sectors were white and black, the black sectors were filled in with India ink.

Observers. Three graduate students in psychology, S. S. Freedman (*F*), R. S. Jaques (*J*), and D. J. Wilson (*W*), served as the *O*s.

Experimental procedure for Series 1, 2, and 3. The 20 cards comprising these three series were presented together. As the diaphragm that formed the contour of the circles was painted black, the form stood out very clearly. The background for the prefixation-field was likewise black. During the prefixation period *O* saw a white circle with a dot in the center; when the fixation-field was illuminated the prefixation-field immediately gave way to a white circular figure with six sectors. The exposure-time for the form was 1 sec. As the preliminary experimentation showed that the perception of 3-sector figures had a temporal course, it was necessary to select an exposure time that would (1) be long enough to give adequate perceptual conditions and permit accurate reporting from the *O*s, and (2) be short enough to prevent reversal of the figures. Between successive presentation 15 sec. elapsed, during which *O* gave his report which was recorded by *E*. In order to facilitate reporting, a duplicate set of 20 cards was drawn. On each of these cards were drawn two circles with sectors corresponding to the cards presented in the tachistoscope. In one circle the big sectors and in the other circle the small sectors were shaded. These two circles were labelled "A" and "B." After each exposure of a form the corresponding checking card was presented to *O* and he reported "A" or "B" according to which set of three sectors he perceived. To prevent *O* from forming any habits of reporting "A" or "B" in a majority of the cases, the circles on the check card with small sectors were in half the cases labelled "A" and in the other half "B," with the opposite procedure for the circles with the large sectors. The instructions given the *O*s were as follows:

> You are going to be shown tachistoscopically a number of circular figures. You are to fixate the dot in the center of the field. About 2 sec. before the field changes and the form appears, I shall say "ready." After seeing the form you will find a card in front of you on the table, on which are drawn two forms, "A" and "B," each form representing one way in which the form presented in the tachistoscope can be seen. If figure "A" on the check card represents what you saw, say "A"; if figure "B" represents what you saw, say "B." If you should experience a reversal of figures during the exposure time report on the basis of the first figure that you saw. Remember that in using the check cards you have two clues in identifying the figure that you saw; namely, the position of the sectors and their size. In one or two of the cards where the sectors appear to approach equality your only clue may be the position of the sectors. After making your report you will refixate the dot and await the next "ready" signal.

The 15-sec. interval between exposure was ample as none of the *O*s was pressed for time; from 6 to 8 sec. elapsed between their report and the next "ready" signal.

Before the experiment proper began each *O* was given about an hour and twenty minutes of training. In the training period *O* made 8 reports on each of the 20 forms (160 exposures). After the series of 20 forms had been presented three or four times the *O*s reported clarity in the perception of the figures and found no difficulty in giving a report of "A" or "B." At times when the 60-60 form was presented only two of the three sectors of a figure would be clearly perceived, but there was no doubt as to which figure the sectors belonged. Graham noted the same phenomenon in his experiment, although here the cause was the prolonged fixation and apparently occurred with several of his forms.[17]

In order to verify the accuracy of the reports, a form was at times left in the tachistoscope and after the *O* had made his report he was referred back to the form on which he had reported, and asked to compare it with the check card to see if any error in designating the figure had been made. No errors of this type were found. In order to discover if the *O*s remembered whether their previous judgments of particular forms had been "A" or "B," they were asked to make reports of "A" or "B" without referring to the check card. Although there were only two possible reports, none of the *O*s scored 50% correct memories. This, of course, did not preclude the possibility of the *O*s remembering whether in a particular form the majority of their reports had been in favor of the small or large sectors. In the extreme cases, such as those where the small sectors were 10°, 20°, and 30°, there was no doubt that they did remember which set of sectors they had been seeing as figure. The possible influence of this memory factor on the results obtained can, however, be more advantageously discussed when the results are dealt with.

In the experiment the set of 20 forms was presented 25 times. In order to guard against fatigue the 25 presentations of the set were divided into 3 sittings. Another safeguard against fatigue was the fact that after the set of 20 forms had been exposed once, there was an interval during which *E* shuffled the cards into a chance order, arranged the checking cards in the same order and rewound the spring kymograph. A longer rest interval was given at the end of each half hour. None of the *O*s reported fatigue or discomfort.

Experimental procedure for Series 4 and 5. As the alternate sectors in these series were black and white it was necessary to have a neutral background, *i.e.* a background midway between black and white. In lieu of the more exact photometric measure, the required gray of the background was determined by equating it subjectively with the midpoint of the Munsell achromatic scale, a standard system employed by artists. The background of the prefixation-field was also painted with the same value of gray.

The *O*s' manner of reporting for this set of 14 forms was much simpler than that employed with the earlier series. They were instructed to report verbally "black" if the black sectors were seen as figure, "white" if the white sectors were seen as figure.

The exposure-time for these two series was reduced to 3/5 sec. as it was found that with the black and white forms the figures were seen more quickly and exposures of 1 sec. permitted reversals. As the *O*s did not require checking cards for these series, and as this decreased the amount of time required by *E* between each exposure, there was only an interval of about 7 sec. between successive presentations of the forms.

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[17] *Op. cit.*, 473.

Before beginning the new black and white series the *O*s were given another training period, but one much shorter than that required for the earlier series.

As in the previous series, the forms for Series 4 and 5 were each presented 25 times. Throughout the entire experiment each *O* made reports on 650 exposures exclusive of training periods.

RESULTS

Series 1. (Small sectors in the indifferent position; see Table I). In all cases where there was a difference in the area of the alternate sectors the small sectors were seen most often as figure. Up to and including the 40-80 form, *W* gave 100% reports in favor of the small-sector figures; for *F* and *J*, 100% reports extended to the 30-90 form. In the remaining forms (excluding from consideration the 60-60 equality form) we find that for all the *O*s an increase in the size of the small sectors is accompanied by a decrease in the number of "small" reports. This tendency was least marked in *W* and most marked in *F*. Despite the decrease in the number of "small" reports, the "small" and "large" reports did not become equal as the sectors approached equality. The increase in "large" reports was not evenly

TABLE I

RESULTS FOR SERIES 1: SMALL SECTORS IN THE INDIFFERENT POSITION

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	22	3	88	12
	55– 65	21	4	84	16
	*60– 60	23	2	92	8
F	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	23	2	92	8
	30– 70	16	9	64	36
	55– 65	14	11	56	44
	*60– 60	17	8	68	32
J	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	22	3	88	12
	50– 70	18	7	72	28
	55– 65	17	8	68	32
	*60– 60	2	23	8	92

* Note that the categories "small" and "large" do not apply to the 60–60 form,

gradated; for all 3 *O*s the greatest single increase in "large" reports occurred between the 40-80 and 50-70 forms. The 60-60 form requires separate comment. Although one might expect that the "small" and "large" reports would approach nearer to equality in this form, this did not occur. *W* and *F* saw one set of alternate sectors in this form 88% and 68% of the times respectively; *J* saw the other set of alternate sectors 92% of the times.

Series 2. (Small sector perpendicular to the base, large sector perpendicular to the top; see Table II). With the small sector perpendicular to the base we find a marked difference in the results. For *W* and *F* there was a decided increase in the number of "small" reports, *W* making 100% reports for all forms in the series in favor of the small-sector figures, and

TABLE II

RESULTS FOR SERIES 2: SMALL SECTORS PERPENDICULAR TO THE BASE; LARGE SECTORS TO THE TOP (Fig. 4)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
W	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	25	0	100	0
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
F	40– 80	25	0	100	0
	50– 70	24	1	96	4
	55– 65	22	3	88	12
	60– 60	21	4	84	16
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	23	2	92	8
J	40– 80	15	10	60	40
	50– 70	10	15	40	60
	55– 65	10	15	40	60
	60– 60	7	18	28	72

F making only 4 "large" reports (exclusive of the 60-60 form) as compared with 22 "large" reports in the previous series. In the 60-60 form *W* reported the figure with sector perpendicular to the base 100% of the times, *F* 84% of the times. We note, then, that with the small sectors perpendicular to the base, there is a marked increase in the number of "small" reports from *W* and *F*. *J*'s results also show a decided difference when com-

pared with his results for Series 1, but a difference, the direction of which is exactly the reverse of that exhibited by *W* and *F*. In this series *J* made 42 "large" reports (exclusive of the 60-60 form) as compared with 18 such reports for the previous series; both in the 50-70 and 55-65 forms the large-sector figures (perpendicular to the top) were dominant (*i.e.* seen more often than the small-sector figures). In the 60-60 form *J's* results are also the reverse of the other two observers, 72% of the reports being in favour of the figure with sector perpendicular to the top. For *W* and *F*, therefore, perpendicularity of a figure to the base adds to the dominance of that figure;[18] for *J* perpendicularity of a figure to the top adds to the dominance of that figure.

TABLE III

RESULTS FOR SERIES 3: SMALL SECTOR PERPENDICULAR TO TOP OF FORM; LARGE SECTOR TO BASE

(Fig. 5)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
W	40– 80	25	0	100	0
	50– 70	24	1	96	4
	55– 65	7	18	28	72
	60– 60	0	25	0	25
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
F	40– 80	21	4	84	16
	50– 70	12	13	48	52
	55– 65	8	17	32	68
	60– 60	4	21	16	84
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
J	40– 80	24	1	96	4
	50– 70	24	1	96	4
	55– 65	23	2	92	8
	60– 60	18	7	72	28

Series 3. (Small sector perpendicular to top, large sector perpendicular to the base; see Table III). Since the forms in this series are simply the reverse of those in Series 2 and serve as a corroborative series, we should expect on the basis of the results already considered that for *W* and *F* there

[18] When a figure is spoken of as being perpendicular to the base or top of a form, it is to be understood, of course, that only one sector of the figure is perpendicular.

would be a marked increase in the number of "large" reports (since for them perpendicularity of a figure to the base adds to its dominance), and that for *J* there would be an increase in the number of "small" reports (since for him perpendicularity to the top adds to a figure's dominance); a reference to Table III will show that such are the actual results. In *W*'s results we find that up to and including the 50-70 form the small-sector figures (at top of form) were still seen in almost 100% of the trials, but that when the sectors are 55-65 the large-sector figures (at base) were seen in 72% of the trials as compared with 16% for Series I and 0% for Series 2. In the 60-60 form the figure with the sector at the base was seen in 100% of the trials. (This result is taken from Series 2, the 60-80 form being identical for both series.) *F*'s results are substantially the same as *W*'s with the exception that for the former large-sector figures (at the base) became dominant with the 50-70 form instead of with the 55-65 form. *J*'s results are again (as they should be) opposite to those of *W* and *F*. For him the small-sector figures when perpendicular at the top were increased in dominance. Referring to Table II it will be noted that for *J*, as for *F*, the large sector figures become dominant with the 50-70 form when the factor of perpendicularity is working against the small-sector figures, whereas the turning point for *W*, as we noted above, was the 55-65 form. This is consistent with, and would appear to corroborate, the indication in the results of Series 1 that the small-sector figures have a greater dominance for *W* than for *F* and *J*.

Summarizing the results of Series 1, 2, and 3 we may say: when the small-sector figures were in the indifferent position all *O*s reported a majority of small-sector figures seen, the number of such reports decreasing as the alternate sectors approached equality. When a figure was perpendicular to the base its dominance was considerably increased for *W* and *F*, even large-sector figures in this position becoming dominant when the area difference was not too great. For *J*, however, perpendicularity of a figure to the top of the form increased the dominance of that figure, large-sector figures in this position becoming dominant when the angular difference of the alternate sectors was 20° or less.

Series 4. (Sectors in the indifferent position, small sectors white, large sectors black; see Table IV). If we compare the results of Series 1 (Table I) with those of Series 4 we find that when the large sectors are black the influence of area is markedly decreased, the large sectors when black becoming dominant over the small-sector figures for *W* when the angular difference of the alternate sectors is reduced to 10°, and becoming dominant for *F* and *J* when the angular difference is 40° or less. This individ-

TABLE IV

RESULTS FOR SERIES 4: SMALL SECTORS IN THE INDIFFERENT POSITION; SMALL SECTORS WHITE; LARGE SECTORS BLACK

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	8	17	32	68
	60– 60	0	25	0	100
F	10–110	25	0	100	0
	20–100	24	1	96	4
	30– 90	16	9	64	36
	40– 80	5	20	20	80
	50– 70	0	25	0	100
	55– 65	0	25	0	100
	60– 60	0	25	0	100
J	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	24	1	96	4
	40– 80	12	13	48	52
	50– 70	2	23	8	92
	55– 65	1	24	4	96
	60– 60	1	24	4	96

ual difference is again consistent with the results of Series 1, 2, and 3 where it was found that the small-sector figures have a greater degree of dominance for *W* than for *F* and *J*.

Series 5. (Sectors in the indifferent position, small sectors black, large sectors white; see Table V). Since we found in Series 4 that blackness added to the dominance of a figure for all 3 *O*s, we should expect in this series, in which the small-sector figures are black, that the dominance of the small-sector figures would be greater than in Series 1 in which both sectors were white; and these are the results that we actually do find. *F* and *J* did not report a single large-sector figure throughout the whole series; *W* also gave 100% report in favor of the small-sector figures up to the 60-60 form where the black figure, although still dominant, was only reported 60% of the times. This series, then, corroborates in a very striking manner, the tendency found in Series 4 for black to add to the dominance of a figure.

Before leaving these results some comment on their reliability should be made. In the previous section, we dealt with certain checks that were used to ensure accuracy in the *O*s' reports and to insure that memories of "A" and "B" reports did not

influence the results. The results of these checks were entirely satisfactory. The question was raised, however, to what extent would the *O*s remember that they had been seeing a certain figure in a specific form, and how would this memory factor influence the results? When the angular difference of the alternate sectors was pronounced, and 100% (or slightly less) reports were made in favor of a certain figure, there was no doubt that the *O*s were aware of the consistency of their reports in favor of that figure. That this memory factor did not influence the results (at least

TABLE V

RESULTS FOR SERIES 5: SMALL SECTORS IN THE INDIFFERENT POSITION; SMALL SECTORS BLACK, LARGE SECTORS WHITE

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
W	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	15	10	60	40
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
F	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	25	0	100	0
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
J	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	25	0	100	0

to any significant degree) can be substantiated, in the writer's opinion, by the fact that in forms for which there was a very great preponderance of reports in favor of a certain figure, the appearance of the second figure on rare occasions was so startling, so strong in its effect and so changed the appearance of the form, that the *O*s spontaneously remarked on the change in their perception. The writer would contend, therefore, that since there is such a striking difference in perception between the two figures of a form, the effect of memory in creating a "set" in favor of the figure most often perceived would be negligible.

Although the forms were presented in a chance order and were shuffled after each set had been presented, there may still be a possibility that the order of presentation affected the results. As it was scarcely feasible to arrange beforehand the order of presentation of the 650 exposures for each *O*, there is no certain means of checking the possible influence of this factor. The raw data of the experiment, however, give certain indications that the order of presentation was not a disturbing

factor. The data for the three sittings during which the forms for Series 1, 2, and 3 were presented together show consistent results for every *O* for each sitting. Since there was a chance order of presentation it is scarcely possible that the first card presented at each sitting, or that the subsequent order of presentation, was the same. Furthermore, we find that the *O*s exhibit the same trends and that these trends are so well marked that the order of presentation could only have caused minor divergencies.

How do the results of this experiment agree with those of other investigations? The general tendency reported by Wertheimer, Rubin, Graham, and others for the small areas to be seen as figure is further substantiated by the results of the present study. It has not been found possible to compare the influence of perpendicularity found in Series 2 and 3 with any other studies; the results, however, appear unequivocal enough. The results for the brightness series differ from those of Graham; he found that the bright, red sectors were seen more often than the darker, blue ones. Using an achromatic scale the present study showed the darker set of sectors to be more dominant. This may possibly substantiate Graham's claim that black creates a special "set" due to past experience with printed matter. It should be noted, however, that although Graham's results with blue and white sectors were not entirely satisfactory, his most reliable and consistent *O* gave reports indicating blue-sector figures to be dominant over white-sector figures; it may be, then, that the dominance of the blue sectors in Graham's study is not due to a color difference, nor the dominance of black sectors in this experiment due to a brightness difference, but that both are due to a specific white and not-white difference in which the not-white figure usually dominates. This, again, may possibly be interpreted in terms of past experience in which white may be said to be our customary ground. Nor is it possible, with the limited amount of data at hand, to conclude that brightness differences in an achromatic scale will always give results that are opposite to those obtained from brightness differences in chromatic scales. The question must remain open for further experimentation.

If one examines the tables for this and Graham's experiment, a striking difference will at once be observed. Graham, because he made use of a fixation and measured dominance in terms of the amount of time that a figure was seen during fixation, naturally found no forms for which a single figure was dominant throughout the entire fixation period; thus with the 10-110 form he found the small-sector figures dominant only about 73% of the time. Furthermore, as the small sectors were increased in size their dominance decreased with fairly even gradations. In the present experiment, in which each form was presented each time as a discrete per-

ception, several forms in the same series often gave 100% results in favour of a certain figure. In contrast with the evenly gradated results of Graham, the results indicate certain critical stages in a series at which abrupt transitions from one figure to another take place, or at which there is a sudden decrease in the number of reports in favour of a figure, although that figure may still remain dominant. It seems fairly certain that these differences in results are due to differences in the methods employed. Which method is best it would be difficult to say. The use of a fixation may have the advantage that it measures finer differences between figures. Finer gradations and the absence of complete dominance of a single figure, may, however, be due to the fact that fixation exaggerates fluctuation from one figure to another in a single form. The method of the present experiment more closely approximates normal perceptual conditions and hence the results obtained might, perhaps, be expected to give a truer picture of the influence of the variable investigated than would results secured by the fixation method.

Conclusions

Since the scope of the various "laws" that determine the perception of figure is relatively unknown, no attempt has been made in this section to draw conclusions that will be applicable to all forms that might be constructed. Only when the many possible kinds of visual forms have been systematically reduced to a limited number of main types, and these have been subject to experimentation, will it be possible to determine the extent to which we are justified in generalizing our conclusions. Experimentation of this nature will certainly have to be undertaken if the laws, principles, factors, etc., enunciated by various investigators are to have any general validity. For the present we content ourselves with the more sober policy of drawing our conclusions in terms of the specific form used in this experiment.

Since in Series 1 the only variable was the area of the sectors in the form, and since the small sectors were seen as figure in the great majority of the times with a corresponding decrease in such reports as the alternate sectors approach equality, we conclude (1) that area is highly effective factor in determining the perception of figure as figures with small areas are on the whole dominant over figures with larger areas—dominance decreasing as difference in area decreases.

When, however, a special positional factor, perpendicularity of the small- and large-sector figures was introduced, it was found that perpendicularity of a figure to the base greatly increased the likelihood that it would be

seen as figure (*W* and *F*), and also that perpendicularity of a figure to the top of the form also increased the likelihood that it would be seen as figure (*J*). Since but 3 *O*s were used we would only tentatively suggest that there may be an "individual difference" in the type of perpendicularity that renders a figure more likely to be perceived. The results, however, are decisive enough to permit the conclusion (2) that perpendicularity of a figure markedly increases the likelihood that that figure will be perceived.

Since in Series 2 for *J*, and Series 3 for *W* and *F* the small sectors were seen as figure up to, but not including, the 50-70 form (*F* and *J*) and 55-65 form (*W*), although the factor of perpendicularity was working against the small-sector figures, and it was not until the alternate sectors approached equality that the factor of perpendicularity became dominant over that of area, it follows (3) that the factor of area exerts a stronger influence than the factor of perpendicularity, as it is only when area-differences are minimized that perpendicularity becomes dominant.

Since in Series 4 and 5 the introduction of black in alternate sectors markedly increased the dominance of the black sectors, these series point to the conclusion (4) that in an achromatic form the introduction of a black-white difference adds to the dominance of the figure that is black. In view of the complicating factors already discussed (*vide supra* 203), this conclusion may be subject to revision with further experimentation.

Since in Series 4 the large, black-sector figures did not become dominant until the angular difference of the alternate sectors had been decreased, it follows (5) that the factor of area exerts a stronger influence than the factor of least brightness, and that only when area differences are minimized does the factor of least brightness become dominant.

The results of this experiment have indicated certain problems that require further experimentation. In view of the marked influence of perpendicularity it would seem highly profitable to inquire more closely into the exact nature of this factor. Was, for instance, perpendicularity of a sector so influential in perception because the sector was perpendicular to the base of the form, or because the sector was perpendicular to the horizontal of the eyes, or because the sector was parallel with the perpendicularity of the bodily posture? What would result if instead of rotating the sectors of the form, the *O*s themselves were rotated?

During the course of the experiment certain individual differences came to light; these would bear further investigation. Did the factor of area have a greater degree of dominance for *W* than for *F* and *J* because of a perseverative factor? Would the following passage from Stevens explain the fact that for *W* and *F* perpendicularity of a sector to the base, and for *J* perpendicularity to the top of the form, increased dominance?

"It will be shown that the eyes of many persons are normally adjusted much above the horizontal plane, that a deviation of 8° or 10° of arc above the horizontal

plane is not very unusual and that there is another large class of persons in whom the deviation of the plane of normal direction of the eyes is materially below the horizontal plane."[19]

As already indicated, the influence of brightness requires investigation to determine whether it is an effective factor in perception *per se*, or whether brightness differences in chromatic and achromatic scales give conflicting results; or, again, whether a brightness difference is just a special case of a more general contrast factor.

Experiments of the present type may also carry wider implications. If it is impossible to generalize one's conclusions from the results based on a specific form, it becomes a primary necessity to reduce visual forms to a restricted number of main types. With these to experiment with, the possible variables should be studied that may determine the perception of form, experimenting with these variables both singly (where possible) and in more and more complex combinations. In this way it might be possible to determine more fully than we now can the factors entering into the perception of the complex visual fields of every-day life, rather than being restricted to the demonstration of the perception of form in the highly simplified figures of the text-book type.

SUMMARY

The purpose of the study was to determine the influence of area, position, and brightness in the visual perception of a reversible configuration, a circle form with six sectors in which the alternate sectors were equal, permitting two figures to be seen. Area was investigated by varying the angles of the sectors, position by varying the position of the sectors in the circle, and brightness by introducing black into the alternate sectors. Measurement of the influence of the variables was in terms of the number of times that each of the two figures in the form was reported seen when exposed tachistoscopically. The results of the experiment show: (1) that figures with small areas are on the whole dominant over figures with larger areas, dominance decreasing as difference in area decreases; (2) that perpendicularity of a figure markedly increases the likehood that that figure will be perceived, but (3) that this factor can make a figure dominant only when the influence of area has been minimized; (4) that in an achromatic form the introduction of a brightness difference adds to the dominance of the figure that is least bright, but (5) that the factor of least brightness can render a figure dominant only when the influence of area has been minimized.

[19] G. T. Stevens, *Motor Apparatus of the Eyes*, 1906, 196.

ASSOCIATION IN RELATION TO FEELING AND GROSS BODILY MOVEMENT

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In 1925 Burtt and Tuttle reported the results of an interesting and suggestive experiment on "The patellar tendon reflex and affective tone."[1] They presented pleasant, unpleasant, and indifferent stimulus-words just prior to elicitations of the patellar tendon reflex, and the extent of the reflex and the association time were measured with suitable apparatus. They found a 16% average reduction in the extent of the tendon reflex for the unpleasant stimulus-words as compared with the indifferent, and the average association-time was slower for the unpleasant stimulus-words than for the indifferent. Their experiment suggested that the depression of the reflex and the slowing of the association-time involved the same aspect of the stimulus-words; namely, the affective tone. The present study is similar in several respects to this investigation by Burtt and Tuttle, and in a general way our results corroborate their findings.

METHOD AND PROCEDURE

We used the word-association method and measured the amount of gross bodily movement which occurred during and for a short time after verbal responses to pleasant (P), indifferent (I), and unpleasant (U) stimulus-words. The 300 different stimulus-words used included the items of the Kent-Rosanoff list (referred to as K-R words), and the *S*s classified each of the stimulus-words as pleasant, indifferent, or unpleasant before the word-association experiment began. During the experiment *S* was seated in a specially constructed chair mounted on springs, and the chair was connected with recording apparatus in such a way that a quantitative measure could be obtained of his gross bodily movements while he was responding to the stimulus-words. The series of stimulus-words was given twice to each *S*, and comparisons were made between the results obtained when the second response was the same as the first and the results obtained when the second response was different from the first. The data which have been secured form an interesting link between several studies of word-association and the expressions of the emotions. The experiment is closely related to the investigations of word-association, physiological complex indicators, expressions of the emotions, and motor psychology;[2]

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[1] H. E. Burtt and W. W. Tuttle, *op. cit.*, this JOURNAL, 36, 1925, 553-61.

[2] Some references to the related literature on word-association may be found, for example, in C. G. Jung, *Studies in Word-Association*, 1919; and C. C. Miles and L. M. Terman, Sex difference in the association of ideas, this JOURNAL, 41, 1929, 165-206.

but the present paper is principally concerned with a description of the experiment itself. Few previous studies have been made of the correlation between feelings and gross bodily movements, and we hope the chair apparatus may prove useful in investigating several of the problems of complex indicators and the detection of guilt.[3]

Material. Three hundred words were used as stimulus-material. One hundred of the words were the Kent-Rosanoff words[4] and the remaining 200 were taken from a list which had been used by the writer in a previous study.[5] The 300 words were mimeographed on 10 pages or data sheets with 30 words on a page, and the last 10 words on each page were from the Kent-Rosanoff list. The first column of the data sheet was used to record the *S*'s judgment of P, I, or U for each word. The stimulus-words were in the second column, and on the right of the stimulus-words were columns in which a record was made of the time of the word-association response, the response word given by the *S*, and the amount of gross bodily movement which occurred during and for a short time after the verbal response. There were 3 other columns on the data sheet in which a record was made of *S*'s later reactions when the stimulus-words were given the second time.

Apparatus. During the experiment *S* was seated in a light but rigidly constructed chair, and the chair was placed in the middle of a platform which was 36 x 42 in. and which was 4½ in. above the floor. The seat of the chair was 20 in. above the platform, and the distance between the arms of the chair was 16½ in. in the back, and 21½ in. in front. The back of the chair sloped at a comfortable angle, and extended 20½ in. above the seat. Each of the chair's four legs rested on a vertically-placed, coil, steel spring, which was 2⅝ in. long and 1⅛ in. in diameter. The spring was securely attached to the platform and to the lower end of the leg of the chair. The distances between the legs of the chair were, for the front legs 15½ in., for the rear legs 14½ in., and for the legs on each side 16½ in. The lower ends of the chair's legs were prevented from moving in any horizontal direction by steel wires which were firmly attached to the lower ends of the legs and to 8 vertically-placed, rigid posts on the edge of the platform. The steel wires were tightly stretched between the two legs on each side, between the front legs, and between the rear legs, and they were continued in all 8 directions to the posts on the edge of the platform. When no one was seated in the chair, the point of attachment of the 4 wires to the lower end of each leg was 4 in. above the surface of the platform and ⅜ in. above the point of attachment to the posts. When a person weighing about 170 lbs. was sitting in the chair, the point of attachment of the 4 wires to the lower end of each leg was 3¼ in. above the platform and ⅜ in. below the point of attachment to the posts. The distance from the legs of the chair to the posts was approximately 11 in. The chair was quite springy to sit in, and moved with practically no friction

[3] Word-association responses and the movements of the hands have been used by Luria in a study of the detection of guilt in criminals. (A. R. Luria, Die Methode der abbildenden Motorik in der Tatbestandsdiagnostik, *Zsch. f. angew. Psychol.*, 35, 1930, 139-83.)

[4] G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insanity*, 67, 1910, 37-96, 317-390.

[5] The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134). The 200 words included all items from "absent" to "generous" in the alphabetical list of words given in Table XII of the above study, with the exception of those words that are in the Kent-Rosanoff list.

in any direction, up and down, forward and back, and sidewise. The springs were strong enough to support a person weighing about 210 lbs. before they were completely depressed.

A wooden strip ¾ in. square extended from the middle of the chair's seat on the lower side directly to the right in a horizontal direction for a distance of 32 in. beyond the right edge of the seat. The inner ends of two other strips of the same material were attached to the front and rear legs 9 in. below the seat, and the outer ends of these 2 strips were securely fastened together and to the outer end of the horizontal strip. The result was a light but rigid arm extending to the right in the horizontal direction a distance of 32 in. beyond the right edge of the seat. A second extension arm constructed in the same way extended from the back of the chair. When the chair moved in any direction, the outer ends of the two extension arms showed a much greater movement, principally in the vertical direction.

Swinging by a string 5 in. long to the end of each extension arm was a piece of hard rubber 9 x 1 x ¼ in., which was free to move up and down in a fixed slide whenever the chair and the extension arms moved. Along the middle line of each piece of hard rubber was a row of 16 round-headed contacts, 5/32 in. in diam. and 5/32 in. apart. A thin strip of copper 2 in. long served as a brush, and the end of this brush touched the contact points on the hard rubber slider whenever the slider moved up and down. The sliders and the outer ends of the extension arms were shielded from the *S*'s view.

The principal features of the chair apparatus were as follows. Any slight bodily movement of the *S* in the chair caused a vertical movement of the two sliders, and produced a number of electric contacts, the number of contacts made being proportional to the amount of gross bodily movement of the *S*.[6] A large number of contacts was made if the *S* shifted his bodily position, moved his leg, raised his arm, or coughed, and some contacts were frequently made when, according to *S*'s retrospective reports, he thought that he had been sitting perfectly still. In most cases the *S* paid very little attention to his gross bodily movements. This apparatus could be made even more sensitive by using smaller round-headed contacts and by placing them closer together.

The recording and timing apparatus was placed on a table some 6 ft. from the *S* and was screened from him. Each of the hard rubber sliders was in circuit with an electric counter and a telegraph relay; and when the recording apparatus was being used, the counters registered the total number of electric contacts made by the sliders during a given interval of time. The electric counters were covered with several layers of sound absorbing material, and placed in front of the dials of the counters were 6 sheets of glass which separated 6 layers of air. *E* could readily read the dials of the counters, but *S* could not hear the counters and did not know that any instruments of this kind were being used.

An apparatus was constructed which made it possible to keep constant the interval of time during which the manual movements of the *S* were being registered by the counters. Current was shunted from the electric light current by means of a laboratory rheostat of 350 ohms capacity, and the shunted current was in circuit

[6] The apparatus which we have devised and described above is similar in some respects to that used by J. S. Szymanski, Aktivität und Ruhe bei den Menschen, *Zsch. f. angew. Psychol.*, 20, 1922, 192-222; and Samuel Renshaw and A. P. Weiss, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

with a telegraph relay and a slow speed motor from a color mixing set. The motor operated a speed reducing system, and a slow moving string belt held taut by a rubber band drew a copper slider, T-shaped in cross section and inverted, along on the surface of a horizontally-placed and slightly convex copper strip. The copper slider moved at a uniform speed and made an electric contact with a projecting horizontal bar after travelling a distance of 3 in. in 5 sec. When the slider was drawn back to its original position by a simple movement of *E*'s left hand, the timing and recording apparatus was ready to be used again.

The general procedure with the apparatus was as follows. *E* spoke the stimulus-word, and at the same time closed a single throw, 3-pole switch with the left hand. One pole of the switch was in circuit with each of the counters registering bodily movements, and the third pole was in circuit with the motor operating the timing apparatus. At the end of a 5-sec. interval the T-shaped copper slider made an electric contact with the projecting horizontal bar and closed a fourth circuit through the break sides of the three telegraph relays, so that the counters registered *S*'s movements only during the 5-sec. interval. One relay broke the circuit through the motor, and each of the two other relays broke a circuit through one of the counters.

While looking at a stop watch, *E* gave the stimulus-word and at the same time closed the 3-pole switch which started the timing and recording apparatus. The verbal response given by *S* and the time of the response were noted by *E*, and the apparatus stopped itself after 5 sec. *E* recorded *S*'s verbal response and the time of the response, and performed the simple calculation necessary to obtain the sum of the differences between the readings on the two counters before and after the 5-sec. interval; 120 trials of this kind could be made without hurrying in one hour, and the apparatus had a definite advantage over some other methods in that all of the data could be recorded without delay.

Almost all of the previous studies of physiological activities involved in feelings and emotions have been concerned with limited portions of the body, and it is of considerable value to determine the correlation between P and U feelings, on the one hand, and gross, random, bodily, or 'manual' movements, on the other, partly because the feelings involve a large mass of bodily tissue in comparison with many other mental activities. On *a priori* grounds it seemed possible to obtain a higher correlation between feelings and gross bodily movements than between feelings and the functioning of any particular portion of the body.

In the present experiment the *S* was seated in the chair apparatus in a natural and comfortable position and was free to move about in any way he desired. Any definite or overt movements of trunk, legs, arms, and head which were the result of contractions in striped or voluntary muscles were measured by the apparatus. The movement record was a measure of the *S*'s fidgetiness, nervous restlessness, physical composure, or persistence of postural attitudes. When the chair did not move at all, *S* was holding himself in position and was maintaining his postural attitudes, and this was a positive response which involved the tonic contractions of a number of striped muscle patterns. The movement records that we secured may not be considered as measures of either relaxation or tension, and one must proceed with care in drawing conclusions in regard to particular muscle groups or specific nervous processes. The measures obtained were measures of gross bodily movements, and we feel obliged to leave their precise physiological interpretation to others.

Procedure. The writer acted as the *E* for all of the *S*s, and the following procedure was used in each case.

(1) *S* was given 1, 2, or 3 mimeographed pages of stimulus-words, and each page contained 30 words arranged in a column. There was a total of 10 different pages, or 300 different stimulus words, and the pages were rotated with different *S*'s so that each page was used 8 times.

(2) *S* was instructed to take in order the words that were to be used with him, to look at each word for a moment, and to grade it P, I, or U according to his personal reactions. The principal basis of judgment was of course the associations that were naturally evoked by the word, but in a few cases the word itself seemed P, I, or U. *S* was asked to proceed through the list slowly enough to give each word a fair chance, but not so slowly that the associations would not be typical and representative of the word. He was asked to spend the same amount of time on the different words, and if possible to give approximately the same number of grades of P, I, and U. After grading the words, *S* was asked to go through the list again and change any grades he desired. He examined the grades once more after an interval of at least one day, and made any further changes in the grades that he considered advisable.

In the large majority of the *S*s the number of grades changed on the second and third gradings combined was less than one grade out of 10. The total number of changes in grades for all of the *S*s was as follows.

		Later grading			
		P	I	U	Total
Earlier grading	P	—	29	10	39
	I	24	—	28	52
	U	21	35	—	56
	Total	45	64	38	147

These figures show that 21 grades were changed from U to P, 10 from P to U, etc.

(3) *S* was seated in the chair apparatus. He was asked to sit in a relaxed and comfortable position and to do anything that he desired provided he carried out the instructions.

(4) He was told to speak the first word he could think of, other than the word spoken by *E*, as quickly as possible after the stimulus-word was given.

During the original gradings of the words as P, I, or U, each word called out the associations that were naturally connected with it, and *S* frequently thought of life situations from his own past experiences. He also frequently thought of life situations from his own past experiences during and after his responses in the word-association procedure. The verbal response was only a part of the total psychological reaction which also included other verbal activities, movements, and feelings. The mental reactions evoked by isolated stimulus words are fairly complicated. "One word used by itself," Downey writes, "is a much richer experience than the same word used in a sentence. The 'word' as a detached consciousness has a tendency to blossom into all manner of images, feelings, impulses."[7] The word-associations in

[7] J. E. Downey, Individual differences in reaction to the word-in-itself, this JOURNAL, 39, 1927, 323-342.

the present experiment were obtained under instructions for speed, and they differ in some respects from those that have sometimes been obtained in a more leisurely manner in some of the studies of complex indicators.

E gave any further directions that were necessary, but nothing was said about *S*'s gross bodily movements or his previous affective classification of the stimulus-words.

(5) The following routine procedure was followed for each trial. (a) While looking at a stop-watch which ran continuously, *E* gave the stimulus-word without a ready signal, and at the same time closed the 3-pole switch which started the timing and recording apparatus. (b) He noted *S*'s verbal response and the time of the response in units of 1/5 sec. (c) The apparatus stopped itself after 5 sec., and at the end of this interval the *S* could hear the sound made by the 3 relays as they broke the circuits through the counters and the motor. (d) *E* opened the 3-pole switch, and drew the T-shaped slider back into position, thereby closing the make sides of the relays again, and preparing the apparatus for the next trial. (e) *E* recorded the response, the time of the response, and the amount of gross bodily movement which had been registered by the two counters during the 5-sec. interval. (f) If *S* did not respond properly or did not respond at all during the 5-sec. interval, *E* gave the stimulus-word again on a later occasion. (g) During the experiment *E* did not pay any attention to which words *S* had previously graded P, I, and U, although these grades could be seen if desired in the left-hand edge of the data sheet. The words were graded quite differently by different *S*s.

(6) After one set of reactions had been obtained to all of the stimulus-words which were to be used with *S*, the experiment was repeated without delay in the same way as before, and the stimulus-words were again taken in the same order. The experiment generally lasted for one hour.

(7) At the end of the experimental period, *S* was asked what he considered the purpose of the experiment to be, what kind of records he thought had been made, and whether he had had any suspicions in regard to what was being investigated. Very few of the *S*s had thought that their manual movements were being measured, but some thought that a record had been made of whether the chair moved some or did not move at all. None of the *S*s thought that a measure had been obtained of the relative speed of word-association responses to P, I, and U stimulus words, or that a comparison would be made between their affective classification of the words and the frequency scores of responses to the K-R words.

Subjects. Most of the 39 *S*s were students in the writer's class in general psychology, and their age and sex distribution was as follows.

Age	17	18	19	20	21	22	23	Total
Women	1	6	8	9	2	0	0	26
Men	0	1	4	2	1	4	1	13

RESULTS

The experimental data are summarized in tabular form, and in some cases the calculations have been made for individual *S*s as well as with the scores from all of the *S*s combined. The customary statistical formulae for variability and reliability have not been used because none of the distributions

is even approximately normal, but the reliability of the results can be fairly accurately judged from the figures in the tables. Movement scores for a few of the trials are lacking because of some difficulty with the apparatus at the beginning of the experiment.[8]

The 300 stimulus-words that were used included the 100 Kent-Rosanoff words and 200 words which the writer had prepared for a previous experiment. The method of selecting these 200 words and the relative number of words that were judged P, I, and U by the present group of *Ss* is of general interest. The procedure originally used in selecting the 200 words was as follows.[9] We began with the 3000 most common words in the English language, according to Thorndike's *Teacher's Word Book* (1921), and reduced the list to 2000 by omitting all words of less than 4 letters, hyphenated words, and proper names. The 2000 words were then classified as P, I, or U by 13 judges. One group of words was made up of those words that were most frequently and consistently judged P, another group of the same size was made up of words that were most consistently judged I, and a third group, also of the same size, was made up of words that were most consistently judged U. These 3 groups of words were then made equal to each other in the number of words generally used as nouns, verbs, and adjectives. The 200 words used in the present study are an alphabetical selection from the above list of words;[10] and the *Ss* in the present experiment should theoretically have given an equal number of P, I, and U grades for these 200 words. According to the judgments of the present group of *Ss*, however, there were for these 200 words 1212 grades of P, 1022 grades of I, and 966 grades of U. For the 100 K-R words which were also used in the present study, there were 702 grades of P, 482 grades of I, and 416 grades of U. The total number of grades of P, I, and U was as follows.

	P	I	U
200 Words	1212	1022	966
100 K-R Words	702	482	416
Total	1914	1504	1382

These results give additional proof of the existence of an important tendency towards optimism in the judgment of affective activities, which we have previously discussed.[11]

(1) Speed of word-association responses. The data on the speed of word-association responses to P, I, and U stimulus-words are given in Tables I and II. Table I shows the relative ranks of P, I, and U stimulus words for different *Ss*, and Table II gives the results obtained by combining the

[8] One of the *Ss*, who was extremely fidgety in the chair, graded practically all of the words either P or U, and also gave the same verbal response on the second trial in practically all cases. It has seemed the most justifiable procedure to omit her results from the calculations of Tables IV and XIII.

[9] The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134), 39-41, 44-46.

[10] *Op. cit.*, Table 12.

[11] *Op. cit.*, 72.

scores from all of the *Ss*. In Table I, for 16 out of 39 *Ss* the responses to the P stimulus-words were slower than to the I or U stimulus-words, but for 13 *Ss* the responses to the P words were faster than to the I or U words. The responses were faster to the I words than to the P or the U words. These results for individual *Ss* are not as reliable as the combined results of the 4800 responses of all of the *Ss* which are given in Table II. In this table all time measures from 15/5 sec. to 25/5 sec. inclusive

TABLE I

RELATIVE RANKS OF P, I, AND U STIMULUS-WORDS IN THE SPEED OF THE FREE ASSOCIATION RESPONSES OF DIFFERENT *Ss*

(Ranks for each S based on median. No. of *Ss* = 39.)

Affectivity	Rank (1 = long time)					Av. Rank	Rank Order
	1	1½	2	2½	3		
P	16	1	8	1	13	1.92	1
I	8		16	3	12	2.14	3
U	14	1	11	2	11	1.94	2

have been counted as 15/5 sec. Counting the time measures above 15/5 sec. at their face values would not have made any difference in the relative values of the central tendencies for P, I, and U. Responses to the U words were slower than responses to the P or I words, but the speed of responses to the P and I words was approximately the same. The U feelings associated with the stimulus words have the effect of slowing up the word-association responses;[12] and this result is in harmony in a general way with the

TABLE II

SPEED OF FREE ASSOCIATION RESPONSES TO P, I, AND U STIMULUS-WORDS: COMBINED RESULTS

(Time in 1/5 sec. No. of *Ss* = 39.)

	Stimulus-Words		
	P	I	U
No.	1914	1504	1382
Av.	6.81	6.80	6.94
Q_1	4.35	4.44	4.56
Q_2	6.27	6.32	6.43
Q_3	8.22	8.15	8.27

view that U activities are relatively slow and have an inhibiting influence on other processes. The slower time of the responses to the U stimulus-

[12] F. L. Wells found that the reactions to visual stimuli in affective settings (*J. Exper. Psychol.*, 8, 1925, 64-76) and the reaction-times to affects accompanying smell stimuli (this JOURNAL, 41, 1929, 83-86) occurred in less than 1 sec. and that there was no difference in the speed of response to these P and U stimuli. The activities which he studied, however, were different from those involved in free word-association.

words probably indicates also that these connections are weaker than those involved in the responses to P or I stimulus-words.[13]

The average speed of response of our *Ss* using a procedure without a ready signal was considerably greater than that obtained by Jung,[14] but

TABLE III

RELATIVE RANKS OF P, I, AND U STIMULUS-WORDS IN THE AMOUNT OF MOVEMENT MADE BY DIFFERENT *Ss* DURING AND AFTER FREE ASSOCIATION RESPONSES

(Ranks for each *S* based on median. No. of *Ss* = 38.)

Affectivity	Rank (1 = large amount of movement)					Av. Rank	Rank Order
	1	1½	2	2½	3		
P	10	1	19	1	7	1.92	2
I	5	2	11	2	18	2.34	3
U	20	3	3	1	11	1.74	1

less than that reported by Burtt and Tuttle.[15] The special problem of the relative speed of word-associations to P, I, and U stimulus-words has been previously studied by Birnbaum, Tolman and Johnson, Anderson and Bolton, and Burtt and Tuttle.[16] Our results agree fairly closely with those obtained by Burtt and Tuttle.

TABLE IV

AMOUNT OF MOVEMENT DURING AND AFTER FREE ASSOCIATION RESPONSES TO P, I, AND U STIMULUS-WORDS: COMBINED RESULTS

(No. of *Ss* = 37.)

	Stimulus-Words		
	P	I	U
No.	1442	1162	1058
Av.	2.65	2.58	2.61
Q_1	1.17	1.14	1.20
Q_2	2.06	2.03	2.08
Q_3	3.46	3.35	3.45

(2) Amount of movement. The results on the amount of movement during and after the responses are given in Tables III and IV. Table III shows that for 18 out of 38 *Ss* less movement occurred during and after responses to the I words than to the P or U words. According to these results, the greatest amount of movement occurred for responses to U words,

[13] See, for example, G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. I, 1900, 1-300.

[14] C. G. Jung, *Studies in Word-Association*, 1919, 227-265.

[15] *Op. cit.*

[16] Karl Birnbaum, Über den Einfluss von Gefühlsfaktoren auf die Assoziationen, *Monatsschr. f. Psychiat. u. Neurol.*, 32, 1912, 95-123, 194-220; E. C. Tolman and I. Johnson. A note on association-time and feeling, this JOURNAL, 29, 1918, 187-195; A. C. Anderson and F. J. Bolton, Inhibition of the unpleasant, *J. Abnorm. & Soc. Psychol.*, 20, 1925-26, 300-302; H. E. Burtt and W. W. Tuttle, *op. cit.*

an intermediate amount for responses to P words, and the least movement occurred for responses to I words. These results are not as reliable as the combined results of all of the *Ss* which are given in Table IV. In this table all movement scores above 10 have been counted as 10. Counting the movement measures above 10 at their face values would not have made any difference in the central tendency values of the movment scores for P, I, and U. In both of the tables, the amount of movement for the responses

TABLE V

RELATIVE RANKS OF P, I, AND U KENT-ROSANOFF STIMULUS-WORDS IN THE FREQUENCY SCORES OF THE RESPONSES OF DIFFERENT *Ss*

(Ranks for each *S* based on median. No. of *Ss* = 39.)

Affectivity	Rank (1 = low frequency score)			Av. Rank	Rank Order
	1	2	3		
P	18	14	7	1.72	1
I	8	13	18	2.26	3
U	13	12	14	2.03	2

to I words was less than the amount of movement for the responses to P or U words. The movement scores during and after responses to the P and U words were approximately the same. It appears therefore that the presence of the affective factor slightly increased the amount of gross bodily movement during and after the responses, and this seems to show that

TABLE VI

FREQUENCY SCORES OF FREE ASSOCIATION RESPONSES TO P, I, AND U KENT-ROSANOFF STIMULUS-WORDS: COMBINED RESULTS

	Stimulus-Words		
	P	I	U
No.	702	482	416
Av.	83.7	109.6	105.9
Q_1	1.4	4.6	2.4
Q_2	22.4	40.5	39.5
Q_3	117.9	186.8	177.1

manual movements are in some way associated with the milder forms of feelings and emotions. These results are similar to those secured by Nunberg with Sommer's apparatus.[17]

(3) Affective responses to the Kent-Rosanoff words. The results on the frequency scores of responses to P, I, and U K-R stimulus-words are given in Tables V and VI. Table V shows, for example, that for 18 out of 39 *Ss* the frequency scores of responses to P words were lower (responses less common) than the frequency scores of responses to U or I

[17] See C. G. Jung, *op. cit.*, 531-560.

words. In Table VI all of the frequency scores from 200 to 650 inclusive have been grouped together, and these frequency scores have been given a value of 325 in calculating the averages. The method of grouping all of the K-R scores is shown in Table XI. The results of Tables V and VI show that the frequency scores of responses to P words are the lowest

TABLE VII

NUMBER OF DIFFERENT RESPONSES GIVEN BY KENT AND ROSANOFF'S *Ss* TO WORDS JUDGED P, I, AND U BY THE PRESENT GROUP OF *Ss*

(Each Kent-Rosanoff word was judged P, I, or U by at least 6 out of 8 of our Ss.)

P				I		U	
K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables
3. music	172	59. health	146	1. table	124	4. sickness	145
5. man	169	61. memory	227	20. chair	106	19. command	214
7. soft	155	63. bath	93	32. citizen	192	31. rough	201
8. eating	168	64. cottage	143	49. eagle	109	34. spider	126
9. mountain	142	66. blue	116	51. stem	120	38. anger	280
13. comfort	169	69. ocean	110	60. bible	128	43. sour	91
16. fruit	126	75. child	157	72. long	178	45. trouble	255
18. smooth	156	79. city	131	77. hammer	109	67. hungry	173
21. sweet	96	86. joy	172	81. butter	101	76. bitter	138
27. river	127	90. baby	151	96. street	163	78. thirsty	86
29. beautiful	182	91. moon	116			83. loud	141
37. sleep	130	94. green	123			84. thief	170
56. justice	178	99. blossom	99			88. heavy	158
58. light	142					100. afraid	189
No.	27				10		14
Av.	144.3				133.0		169.1
Q_1	121.2				107.5		132.0
Q_2	143.0				122.0		164.0
Q_3	169.0				170.5		207.5

(responses most unusual), the frequency scores of responses to U words are next in order, and the frequency scores of responses to I words are the highest (responses most common). The reliable order of rank for peculiarity of the responses to the K-R words is P-U-I, but the difference between the scores for P and U is much greater than the difference between the scores for U and I. It seems justifiable to assume that the responses associated with the P stimulus-words are the weakest and the least habitual.

In Table VII we have brought together all of the *K-R words* that were *judged P, I, or U* by at least 6 out of 8 of our *Ss*. On the left of each K-R word is the number of the word as it appears in the K-R tables, and in the column to the right of each K-R word is the number of different responses given to this stimulus-word by all of the 1000 K-R *Ss*. For example, the

third word in the K-R list is "music," and Kent and Rosanoff's *Ss* gave 172 different responses to this word. Although the total number of words used in the present calculation is only 51, it appears that the amount of scatter, as measured by the number of different responses given by Kent-Rosanoff's *Ss*, is greatest for the U words, second in order for the P words, and least for the I words.

TABLE VIII

CASES OF FAILURE TO RESPOND

Number and percent of failures to respond to the different stimulus-words			Amount of movement during failure to respond		Amount of movement during and after later response		Percent of later changed responses after *S* failed to respond on first trial
P (1914)	I (1504)	U (1382)	Av.	Med.	Av.	Med.	
25 1.3%	32 2.1%	27 2.0%	3.6	2.0	2.6	2.0	48.3%
N=84			N=79		N=84		N=60

(4) *Failures to respond.* When *S* did not respond during the 5-sec. interval, *E* noted the fact and in most cases made a record of the amount of movement occurring during the interval. The data on these cases are given in Table VIII, but they are not particularly reliable because of the small number of cases. Table II shows that 1914 words were judged P, 1504

TABLE IX

RELATIVE RANKS OF P, I, AND U STIMULUS-WORDS IN THE PERCENTAGE OF RESPONSES CHANGED BY DIFFERENT *Ss* ON THE SECOND FREE ASSOCIATION RESPONSE

(No. of *Ss*=39.)

Affectivity	Rank (1=large percentage changed)				Av. Rank	Rank order
	1	1½	2	3		
P	17	1	13	8	1.76	1
I	8	1	12	18	2.24	3
U	12	2	12	13	2.00	2

were judged I, and 1382 were judged U, when all of the stimulus-words and *Ss* are combined. If the calculation is based on the total number of judgments of P, I, and U, there was only 1.3% of failures to respond to the P words, as compared with 2.1% of failures to respond for the I words and 2.0% for the U words. The average amount of movement which occurred during the failures to respond (3.6) was greater than that which occurred in an equal interval of time during and after the responses (2.6, in Table IV); but the median amount of movement which occurred during the failures to respond (2.0) was approximately the same as that which

ordinarily occurred during and after the responses (2.1, in Table IV). There was no difference between the amount of movement during and after the responses that were obtained later, after failure to respond, and

TABLE X

PERCENTAGE OF RESPONSES CHANGED ON THE SECOND FREE ASSOCIATION RESPONSE TO P, I, AND U STIMULUS-WORDS: COMBINED RESULTS

(No. of Ss = 39.)

Responses changed	Stimulus-Words		
	P	I	U
Percentage	51.9%	49.5%	50.4%
No.	957	752	691

the amount of movement which ordinarily occurred; and there was also no reliable difference between the percentage of later changed responses after *S* failed to respond on the first trial (48.3) and the percentage of responses that were ordinarily changed (50.6, in Table X).

(5) *Change at second response.* The percentage of responses that were changed at the second word-association response to P, I, and U words are given in Tables IX and X. A slightly larger percentage of responses was changed to the P stimulus-words than to the U words, and a slightly larger percentage of responses was changed to the U words than to the I words. These results suggest that the general stability or consistency of the responses was highest for the I words and lowest for the P words.

It may be recalled that each *S* graded the stimulus-words that he was to use as P, I, or U according to his own personal reactions. The feelings associated with most of the words were quite mild, but this seems to make the results all the more important because mild feelings are the kind of feelings that are most commonly associated with our language responses. The experimental data which we have obtained therefore afford a useful basis of comparison between the results of a number of experiments on word-association, on the one hand, and several studies of complex indicators and the expressions of the emotions, on the other. The principal results which we have described above may be briefly summarized as follows.

Speed (Slowest first)	Movement (Greatest first)	K-R Score (Lowest first)	Responses Changed (Largest percentage first)	No. Diff. Responses in K-R Tables (Largest number first)
U	U P	P	P	U
		U	U	P
P I	I	I	I	I

Further data on the relations between the number of changed responses, on the one hand, and the frequency scores of the K-R words, the speed of the responses, and the amount of movement during and after the responses, on the other, are given in Tables XI, XII, and XIII. Each of the stimulus-words used with a given *S* was used twice; and for each stimulus-word there were two responses, two measures of the speed of the response, and two measures of the amount of movement during and after

TABLE XI

RELATION BETWEEN THE FREQUENCY SCORES OF RESPONSES TO THE KENT-ROSANOFF WORDS AND THE NUMBER OF CHANGED RESPONSES

K-R frequency scores	K-R frequency score of: First response			Second response		
	When second same	When second different	% change	When same as first	When different from first	% change
200–650	113	59	34.3	113	31	21.5
100–199	56	49	46.7	56	50	47.2
50– 99	50	39	43.8	50	36	41.9
25– 49	35	34	49.3	35	41	53.9
15– 24	22	17	43.6	22	28	56.0
10– 14	15	16	51.6	15	23	60.5
7– 9	8	20	71.4	8	17	68.0
5 and 6	13	13	50.0	13	14	51.9
3 and 4	11	20	64.5	11	18	62.1
1 and 2	28	55	66.3	28	49	63.6
0	36	91	73.4	36	106	74.6
No.	387	413		387	413	
Av.	113.8	76.6		131.8	55.6	
Q_1	9.8	0.9		9.8	0.5	
Q_2	75.0	11.8		75.0	10.0	
Q_3	227.5	109.2		227.5	68.6	

the response. In some cases the response given on the second trial was the same as the response given on the first trial, and in other cases the second response was different from the first. In the data presented in Tables XI, XII, and XIII, we have made comparisons between the results on frequency, time, and movement when the responses on the two occasions were the same and when the responses on the two occasions were different.

Table XI shows the relation between the frequency scores of responses to the K-R words and the number of responses that were changed on the second trial. We have made one calculation using the K-R frequency scores of the first of the two responses, and another calculation using the K-R frequency scores of the second of the two responses. Considering first the K-R frequency scores of the first of the two responses, for those responses

TABLE XII

RELATION BETWEEN SPEED OF RESPONSE AND THE NUMBER OF CHANGED RESPONSES

Time in ¼ sec.	Time of ▪ First response: When second same	First response: When second different	First response: % change	Second response: When same as first	Second response: When different from first	Second response: % change
15+	22	75	77.3	17	48	73.8
14	10	31	75.6	8	29	78.4
13	16	50	75.8	7	37	84.1
12	21	46	68.7	15	42	73.7
11	33	70	68.0	40	75	65.2
10	28	48	63.2	18	24	57.1
9	41	66	61.7	24	71	74.7
8	146	189	56.4	108	211	66.1
7	145	172	54.3	111	179	61.7
6	215	184	46.1	159	193	54.8
5	166	108	39.4	152	101	39.9
4	195	102	34.3	206	126	38.0
3	140	83	37.2	313	86	21.6
No.	1178	1222		1178	1222	
Av.	6.32	7.87		5.60	7.51	
Q_1	4.29	5.57		3.44	5.43	
Q_2	5.91	7.28		4.96	7.09	
Q_3	7.65	9.76		6.98	8.79	

TABLE XIII

RELATION BETWEEN AMOUNT OF MOVEMENT AND THE NUMBER OF CHANGED RESPONSES

Amount of Movement	Movement during and after First Response: When second same	First Response: When second different	First Response: % change	Second Response: When same as first	Second Response: When different from first	Second Response: % change
10+	16	19	54.3	15	20	57.1
9	9	16	64.0	4	8	66.7
8	12	15	55.6	9	21	70.0
7	17	34	66.7	17	20	54.1
6	39	37	48.7	29	31	51.7
5	56	51	47.7	43	58	57.4
4	62	80	56.3	53	79	59.8
3	114	139	54.9	95	120	55.8
2	230	321	58.3	263	294	52.8
1	230	232	50.2	215	231	51.8
0	73	72	49.7	90	73	44.8
No.	858	1016		833	955	
Av.	2.62	2.74		2.41	2.67	
Q_1	1.12	1.28		1.05	1.22	
Q_2	2.05	2.14		1.92	2.09	
Q_3	3.47	3.49		3.10	3.49	

that were 'individual' in the K-R tables (frequency of zero), there were 36 cases where the second response was the same as the first, and 91 cases where the second response was different from the first, or 73.4% of changed responses. For those responses that had a K-R frequency score of 200 or more, there were 113 cases where the second response was the same as the first, and 59 cases where the second response was different from the first, or 34.3% of changed responses. There is a tendency for the percentage of changed responses to increase as the K-R frequency scores decrease. The average frequency score of the same responses is 131.8, and the average frequency score of the different responses is 76.6; while the median frequency score of the same responses is 75.0, and the median frequency score of the different responses is 11.8. Similar results are obtained when the calculations are based on the K-R frequency scores of the second of the two responses. The results indicate that the K-R frequency scores are less common when the second response is different from the first than when the second response is the same as the first.

Table XII shows the relation between the speed of the response and the number of responses that were changed on the second trial. Considering the time of the first response, for those responses that had a time of 15/5 sec. or more, there were 22 cases where the second response was the same as the first, and 75 cases where the second response was different from the first, or 77.3% of changed responses. For those responses that had a time of 3/5 sec., there were 140 cases where the second response was the same as the first, and 83 cases where the second response was different from the first, or 37.2% of changed responses. There is a tendency for the percentage of changed responses to increase as the time increases. The average time of the same responses is 6.32/5 sec., and the average time of the different responses is 7.87/5 sec.; while the median time of the same responses is 5.91/5 sec., and the median time of the different responses is 7.28/5 sec. Similar results are obtained when the calculations are based on the time scores of the second of the two responses. These results indicate that the speed of the response is less, and the connections are probably weaker, when the second response is different from the first than when the second response is the same as the first.[18]

Table XIII shows the relation between the amount of movement and the number of responses that were changed on the second trial. If we consider the amount of movement that occurred during and after the first response, for those responses where the movement score was at least 10, there were 16 cases where the second response was the same as the

[18] A similar tendency was observed by M. Pappenheim, Merkfähigkeit und Assoziationsversuche, *Zsch. f. Psychol.*, 46, 1908, 161-173.

first, and 19 cases where the second response was different from the first, or 54.3% of changed responses. The average movement score of the same responses was 2.62, and the average movement score of the different responses was 2.74, while the medians were 2.05 and 2.14, respectively. Similar results are obtained when the calculations are based on the amount of movement that occurred during and after the second of the two responses. These results indicate that the amount of movement is slightly greater when the second response is different from the first than when the second response is the same as the first.

The results given in Tables X–XIII show that there was avoidance of repetitive responses in approximately one-half of the cases, and the results in Table XIII on the first and second responses to the K-R words indicate that the responses became somewhat more unusual on the second trial. These results seem to have been influenced by the *S*s' general habit of avoiding repetition in ordinary language activities; and it appears that they cannot be explained by reference to the physiological principle of refractory phase.[19] In obtaining the second responses to the stimulus-words, the *S* was asked to speak the first word that came to mind regardless of whether it was the same as, or was different from, the response that he had given on the first trial, but the attempt to avoid all factors of suggestion in these instructions may not have been entirely successful.

After one response had been obtained to each of the stimulus-words that were to be used with an individual *S*, a second response was obtained to the same list of stimulus-words, and the stimulus-words were taken in the same order as before. The results given in the left blocks of Tables XI, XII, and XIII were obtained in the first half of the experimental period and the results in the right blocks were obtained in the last half of the experimental period.

In Table XI, on the frequency scores of responses to the K-R words, the only comparison that can be made between the two halves of the experiment, *i.e.* between the first and second responses to the same stimulus-word, is when the second response was different from the first. The frequency scores based on the first and the second responses to each stimulus-word, are as follows.

	Different Responses	
	First	Second
Av.	76.6	55.6
Q_1	0.9	0.5
Q_2	11.8	10.0
Q_3	109.2	68.6

[19] Cf. C. W. Luh, The avoidance of repetitive responses, *Psychol. Rev.*, 38, 1931, 276-9.

These results show that the frequency scores of the second responses to the K-R stimulus-words were lower (the responses less common) than the frequency scores of the first responses to the K-R stimulus-words.

The data from Table XII, on the time of the first and second responses are as follows.

	Same Responses		Different Responses	
	First	Second	First	Second
Av.	6.32	5.60	7.87	7.51
Q_1	4.29	3.44	5.57	5.43
Q_2	5.91	4.96	7.28	7.09
Q_3	7.65	6.98	9.76	8.79

These results show that the second responses to the stimulus-words were faster than the first responses to the same stimulus-words.

The data from Table XIII, on the amount of movement during and after the first and second responses are as follows.

	Same Responses		Different Responses	
	First	Second	First	Second
Av.	2.62	2.41	2.74	2.67
Q_1	1.12	1.05	1.28	1.22
Q_2	2.05	1.92	2.14	2.09
Q_3	3.47	3.10	3.49	3.49

These results show that less movement occurred during and after the second responses in the second halves of the experimental period than during and after the first responses to the same stimulus-words in the first halves of the experimental period.[20]

(6) Relation between frequency scores, time, and amount of movement. The results on the relation between the frequency scores of responses to the K-R words, the time of the responses, and the amount of movement during and after the responses are given in Tables XIV, XV, and XVI; and the construction of all three tables is the same.

In Table XIV on the relation between frequency scores and time, there were 80 cases where the frequency score of the response was at least 200 and where the time was 3/5 sec. The total number of cases is shown in the third row from the bottom and in the third column from the right. The median scores are given in the second row from the bottom and in

[20] Some of these results on the effect of practice on word-association corroborate the findings of F. L. Wells, Practice effects in free association, this JOURNAL, 22, 1911, 1-13.

TABLE XIV

RELATION BETWEEN THE FREQUENCY SCORES OF FREE ASSOCIATION RESPONSES TO THE KENT-ROSANOFF STIMULUS-WORDS AND THE SPEED OF THE RESPONSES

K-R frequency scores	Time in ⅕ Sec.													No. cases	Time Med.	Time Av.	
	3	4	5	6	7	8	9	10	11	12	13	14	15+				
200–650	80	56	34	61	35	26	4	5	7	1	4		3	316	5.1		
100–199	45	50	30	19	17	30	3	5	6	3	1		2	211	4.9	5.2	
50– 99	38	28	17	33	15	22	4	1	6	3	2	4	2	175	5.6		
25– 49	27	14	19	28	11	22	10	6	4		3		1	145	5.9		
15– 24	10	13	14	7	15	14	5	2	2	1	2	2	2	89	6.5		
10– 14	6	13	9	14	12	6	2	1	2		3	1		69	6.0	6.4	
7– 9	2	6	6	9	9	15	1	2		1	1		1	53	6.9		
5 and 6	4	6	6	9	13	5	1	2	3	1	1		2	53	6.6		
3 and 4	6	3	6	16	8	12	1	2	1		3	1	1	60	6.4		
1 and 2	19	13	14	36	18	22	8	2	6	5	9	4	4	160	6.4	6.6	
Individ.	17	28	27	40	35	46	13	8	12	17	9	4	13	269	7.1		
No. cases	254	230	182	272	188	220	52	36	49	32	38	16	31	1600			
K-R frequency scores: Med.	97.0	83.5	36.3	29.0	14.1	17.4	14.5	19.5	17.0	4.7	3.2	2.5	1.8				
Av.		61.4				16.4					5.8						

the second column from the right. The median frequency score for the time of 3/5 sec. is 97.0, and the median time score for the frequency of 200-650 is 5.1/5 sec. In the bottom row, the figure 61.4 is the average of the four medians, 97.0, 83.5, 36.3, and 29.0. The results in the table indicate that as the time increases the frequency scores decrease, and as the frequency scores decrease the time increases. This finding supports a result that we have previously obtained.[21]

Table XV shows the relation between the frequency scores and the amount of movement, and it is interesting to note that the correlation between the two is practically zero. The results on the relation between time and movement are given in Table XVI, and these figures likewise show that there is practically a zero correlation between the two. In Tables XIV, XV, and XVI, slightly different results would have been obtained if the averages instead of the medians had been calculated in the second row from the bottom and in the second column from the right, but all of the scores are given in the tables and the most accurate impression may be formed by a direct examination of the figures themselves.

[21] H. and E. B. Cason, Association tendencies and learning ability, *J. Exper. Psychol.*, 8, 1925, 167-89. Cf. also M. Trautscholdt, Experimentelle Untersuchungen über die Assoziation der Vorstellungen, *Phil. Stud.*, 1, 1883, 213-250.

TABLE XV

RELATION BETWEEN THE FREQUENCY SCORES OF FREE ASSOCIATION RESPONSES TO THE KENT-ROSANOFF STIMULUS-WORDS AND THE AMOUNT OF MOVEMENT DURING AND AFTER THE RESPONSES

K-R frequency scores	Amount of Movement											No. cases	Movement Med.	Movement Av.
	0	1	2	3	4	5	6	7	8	9	10+			
200–650	26	64	82	29	15	12	9	10	4	5	9	265	2.0	
100–199	13	50	53	23	10	8	5	3	2	2	7	176	2.0	2.1
50– 99	14	27	45	15	11	6	7	3	3	1	9	141	2.2	
25– 49	14	42	30	13	5	8	3		3	1	1	120	1.6	
15– 24	5	17	26	10	5	1	4	4			7	79	2.2	
10– 14	8	13	14	1	7	2		1	2		1	49	1.8	1.9
7– 9	2	13	17	3	4	2		1			1	43	1.9	
5 and 6	5	8	12	5	3	2	2				4	41	2.1	
3 and 4	1	11	15	7	3	1	2	4	1			45	2.2	
1 and 2	10	28	33	16	13	6	8	2	2	1	4	123	2.2	2.2
Individ.	15	53	66	33	15	11	7	8	5	2	12	227	2.2	
No. cases	113	326	393	155	91	59	47	36	22	12	55	1309		
K-R frequency scores: Med.	43.3	36.5	35.8	29.3	15.5	38.5	28.8	19.5	32.8	149.5	22.4			
Av.	38.5			27.8			50.6							

TABLE XVI

RELATION BETWEEN SPEED OF FREE ASSOCIATION RESPONSES AND THE AMOUNT OF MOVEMENT DURING AND AFTER THE RESPONSES

Time in ⅕ sec.	Amount of Movement											No. cases	Movement Med.	Movement Av.
	0	1	2	3	4	5	6	7	8	9	10+			
15+	6	27	44	19	9	3	8	2	1	3	4	126	2.2	
14	5	17	20	5	10	1	2				2	62	2.0	
13	5	21	24	13	7	6	7	3		2	4	92	2.3	2.1
12	8	19	38	12	6	7	6	3			4	103	2.1	
11	7	44	60	21	9	12	9	10	2	1	1	176	2.1	
10	6	23	30	14	7	3	5	4	2	1	2	97	2.2	
9	12	37	45	27	16	11	6	1	2	3	5	165	2.2	2.1
8	34	131	149	61	45	30	20	14	13	6	11	514	2.1	
7	37	117	150	57	43	24	15	10	3	5	19	480	2.1	
6	47	146	186	78	36	39	19	13	11	3	27	605	2.1	
5	50	109	107	54	26	27	16	11	11	5	12	428	2.0	2.0
4	54	119	148	65	36	32	16	16	15	7	18	526	2.1	
3	51	149	141	60	39	22	20	11	9	4	22	528	2.0	
No. cases	322	959	1142	486	289	217	149	98	69	40	131	3902		
Time: Med.	5.6	6.2	6.4	6.3	6.7	6.2	6.7	6.3	5.5	6.7	6.0			
Av.	6.1			6.4			6.2							

Summary and Conclusions

The *S* was seated in a specially constructed chair apparatus, and the amount of gross bodily movement was measured during and for a short time after word-association responses to pleasant, indifferent, and unpleasant stimulus words. The list of stimulus-words was repeated, and various comparisons were made between speed of responses, amount of gross bodily movement, frequency scores of responses to the Kent-Rosanoff words, and number of responses that were changed on the second trial. The results of the experiment are closely related to the investigations of word-association, complex indicators, expressions of the emotions, and motor psychology. The principal conclusions are as follows.

(1) The *S*s showed a tendency towards optimism in grading the words P, I, and U.

(2) The responses to the U stimulus-words were slower than the responses to the P or I stimulus-words, but the speed of the responses to the P and I stimulus-words was approximately the same.

(3) Less gross bodily movement occurred during and after the responses to the I stimulus-words than during and after the responses to the U or P stimulus-words.

(4) The order of rank for peculiarity of the responses to the K-R words was P-U-I.

(5) The order of rank for the percentage of responses changed on the second trial was P-U-I.

(6) There was a tendency for the percentage of responses changed on the second trial to increase as the frequency scores of the responses to the K-R words decreased.

(7) There was a tendency for the percentage of responses that were changed on the second trial to increase as the time of the responses increased.

(8) More movement occurred during and after the responses when the second response was different from the first than when the second response was the same as the first.

(9) When the word-association procedure was repeated, there was avoidance of repetitive responses in approximately one-half of the cases.

(10) For the second of the two responses to the stimulus-words, *i.e.* during the second halves of the experimental periods, the frequency scores of responses to the K-R words were lower (the responses were less common), the speed of the responses was faster, and less movement occurred during and after the responses.

(11) There was a tendency for the frequency scores of responses to K-R words to decrease as the time of the responses increased.

(12) The correlation between the frequency scores of responses to the K-R words and the amount of movement which occurred during and after the responses was approximately zero.

(13) The correlation between the time of the responses and the amount of movement which occurred during and after the responses was also approximately zero.

ADAPTATION TO THE PAIN OF RADIANT HEAT

By LAWRENCE JOSEPH STONE, Columbia University, and
KARL M. DALLENBACH, Cornell University

The controversy regarding the essential nature of pain which has long been waged by Von Frey and Goldscheider,[1] is concerned with one of the fundamental problems in sensory physiology and psychology. Its resolution is of systematic importance to both sciences.

Von Frey holds that cutaneous pain is a separate modality with specific nervous structures—*i.e.* the free nerve-endings. Included in the evidence which he offers for this theory are his results from an experiment on pain adaptation. He found that, under continuous stimulation, a painful experience becomes steadily weaker and finally disappears without undergoing a qualitative change.[2]

Goldscheider, on the other hand, regards pain as a common feeling (*Gemeingefühl*) without specific nerve-endings. It is aroused, he thinks, through the mediation of the tactile nerve fibers and results from a summation of their excitations: weak stimulation gives touch; intense stimulation, by way of summation, gives pain. In support of this theory he too points to the results of an experiment on pain adaptation. He found, under continuous stimulation, that the experience which is at first painful not only becomes weaker but also undergoes a qualitative change;[3] it passes, he reports, into an *"unterschmerzliche Empfindung"* or *"Druck"* before its complete disappearance.

Each side of the controversy points to the phenomenon of pain adaptation as evidence for its theory; consequently the problem of adaptation has become an *experimentum crucis*.

Two studies have recently appeared upon the adaptation of cutaneous pain. Besides confirming the fact of adaptation, they attempted to resolve this controversy. In the first study, Wells and Hcisington found that "the end-quality of pure, non-pressury, cutaneous pain . . . is neither a weak pain (Von Frey) nor a pressure

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[1] For an exposition of the two points of view and a history of the controversy see Friedrich Kiesow, The problem of the conditions of arousal of the pure sensation of cutaneous pain, *J. Gen. Psychol.*, 1, 1928, 199-212.

[2] M. von Frey, Beiträge zur Physiologie der Schmerzsinnes, *Ber. ü. d. Verhandl. d. königl. sächs. Ges. d. Wiss. z. Leipzig, math.-phys. Kl.*, 46, 1894, 193; *Untertersuchungen über die Sinnesfunktionen der menschlichen Haut*, 1896, 242; Der Schmerzsinn, in M. von Frey and H. Rein's, *Physiologie der Haut*, 1929, 142 (Vol. 1, Pt. 2, of the *Handbuch der Haut- und Geschlechtskrankheiten*, 1929).

[3] A. Goldscheider, *Gesammelte Abhandlungen*, 1, 1898, 44, 397; Über Schmerz und Schmerzbehandlung, *Zsch. f. physik, und diät. Therap.*, 19, 1915, 289-309; *Das Schmerzproblem*, 1920, 20, 71; Beiträge zur Physiologie der Gemeingefühle: I. Physiologie des Schmerzes, *Zsch. f. Sinnesphysiol.*, 57, 1926, 1 ff.

(Goldscheider), but a non-pressury 'bright contact'."[4] Upon the basis of this result, they suggest that the difference between Von Frey and Goldscheider is principally terminological.

Though recognizing the difficulty and danger of equating qualitative terms, Wells and Hoisington suggest, by way of reconciling the disparate results, that Von Frey's "weak pains" and Goldscheider's *"unterschmerzliche Empfindungen"* and *"Drücke"* correspond to their "bright contacts." If such is the case—"if Von Frey's insistence that isolated pain spots give nothing but pain is . . . due to a confusion of 'weak pain' with non-painful 'bright contact'," and "if Goldscheider's contention that isolated pain spots give 'pressure' is the result of a failure to differentiate between true pressure and the non-pressury contact-like experience"—then, they conclude, "the controversy will be settled."[5] Yes, settled by the elimination of Von Frey! For Von Frey to admit "that the final phase of pain subsidence is not really painful"[6] (as Wells and Hoisington would have him do), would be to accept Goldscheider's theory, for such a statement contains the essence of the latter's theory. Wells and Hoisington's study must, therefore, be regarded as corroborating Goldscheider.

The second study, by Burns and Dallenbach,[7] also corroborates Goldscheider, for these authors found that "the pain experiences aroused by punctiform stimuli are replaced by pressury experiences before the stimulus is completely adapted."[8] Their confirmation, however, is conditional, since they recognize the possibility, which others have overlooked, that the replacement of pain by pressure or 'bright contact' might be due to "the residual effect of the pressure-pain stimulus" employed in their experiments.[9] They are of the opinion that this is the case; but, as they observed, "the only way . . . of deciding this question is to arouse pain with stimuli, for example, radiant heat, that do not give rise simultaneously to pressure experiences."[10] This statement provides the point of departure for the present study.

Our object in this study was twofold. We wished (1) to learn whether the pain aroused by radiant heat would under continuous stimulation come to complete adaptation; and (2) to discover whether pressure or pressure-like experiences were inevitable concomitants of pain at any phase in the process. We hoped to be able, in the light of our results, to contribute something definite to the broader problem with which Von Frey and Goldscheider were concerned; namely, the fundamental nature of pain: is it a separate modality, or is it merely part and parcel of a *Gemeingefühl* which includes pressure?

Radiant heat has not, as far as we know, been used in the study of pain adaptation, though it has been used as a stimulus for the arousal of

[4] E. F. Wells and L. B. Hoisington, Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.*, 1, 1928, 364.
[5] *Op. cit.*, 365.
[6] *Idem.*
[7] M. Burns and K. M. Dallenbach, The adaptation of cutaneous pain, this JOURNAL, 45, 1933, 111-117.
[8] *Ibid.*, 117.
[9] *Idem.*
[10] *Ibid.*, 116.

pain.[11] Von Frey used it in testing Goldscheider's statement "dass die Erregung der Schmerzpunkte *unterschmerzlich beginnt und unterschmerzlich aufhört.*"[12] In order to escape the concomitant pressures that are aroused when an object is brought to bear upon the skin, Von Frey aroused pain by concentrating the sun's rays upon the tissues by means of a lens. Because he was able to obtain pure, pressureless pains by this method, he concluded that Goldscheider's summation theory of pain is untenable.[13] In refutation of this experiment and conclusion, Goldscheider pointed out that the course of experience is telescoped when strong stimuli are used, and that the *unterschmerzliche Empfindungen* (*Berührungsempfindungen*) are present, though obscured by the intense pain.[14] "Ich habe," says Goldscheider, "deshalb den Verbrennungschmerz gar nicht in den Kreis meiner Untersuchungen einbezogen und es wäre besser gewesen, wenn auch mein Gegner auf dieses unzureichende Beweismaterial verzichtet hatte."[15]

Fortunately, in refuting Von Frey's evidence, Goldscheider lays down, by implication at least, the conditions under which the *unterschmerzliche Empfindungen* are to be perceived. They are two in number: (1) the pain aroused should not be intense; and (2) it should not rise suddenly or quickly. Though these conditions may not have been realized in Von Frey's experiments with radiant heat, they did obtain, as we shall later show, in our experiments. Consequently, if we discover that subpainful pressury experiences are not invariable accompaniments of the pain of radiant heat, then Burns and Dallenbach's suggestion—that the pressury concomitants of painful experiences are due to the pressure-pain stimulus employed—will be substantiated, and Von Frey's theory that pain is a distinct and separate modality of sensation will be confirmed.

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[11] It has frequently been used as a stimulus in studies on the localization of pure warmth. For a report of such an experiment and a review of the earlier literature, see F. L. Dimmick, On the localization of pure warmth sensations, this JOURNAL, 26, 1915, 142-150.

[12] Goldscheider, *Das Schmerzproblem*, 1920, 21.

[13] Von Frey writes of these pressureless pains as follows: "Für die vorliegende Betrachtung ist wesentlich, dass an vielen Orten Schmerz ohne begleitende andersartige Empfindung erregt werden kann, insbesondere fehlt jede Berührungsempfindung, die angeblich den Schmerz regelmässig begleiten oder ihm vorausgehen soll" (Versuche über schmerzerregende Reize, *Zsch. f. Biol.*, 76, 1922, 9).

[14] Regarding the statement, "dass eine Berührungsempfindung den Schmerz regelmässig begleiten oder ihm vorausgehen soll," with which Von Frey credits him, Goldscheider writes: "Dies gilt aber . . . nicht für Reize von solcher Stärke, dass die Erregung das Stadium der Berührungsempfindung schnell durchlaufend sofort zur schmerzhaften Höhe ansteigt. Maximale Schmerzreize sind überhaupt ganz ungeeignet, um in dieser Frage etwas zu entscheiden, da der plötzlich einbrechende starke Schmerz alle anderen Empfindungen überlagert" (Beiträge zur Physiologie der Gemeingefühle: I. Physiologie des Schmerzes, *Zsch. f. Sinnesphysiol.*, 57, 1926, 5).

[15] *Idem.*

Method and Procedure

The dorsal surfaces of both forearms were used as the experimental tissues. These areas were shaved and kept smooth during the course of the experiment, so as to avoid the pressury experiences which otherwise might have been aroused by contact of the hairs with the radiant-heat stimulator.

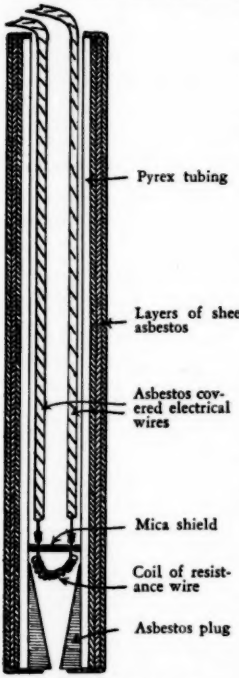

Fig. 1. Diagram of Stimulator

At the beginning of an experimental period, *O* seated himself in a comfortable chair placed sidewise at the end of a table. The arm (hand and forearm) next to the table was placed behind a screen, and in a plaster of Paris arm-rest that had been cast especially for it. The experimental area was thus kept relatively free from accidental movements and tremors.

The back of the forearm was roughly divided into quarters, and a given quarter was stimulated only once during an experimental period. After every two trials *O* was given a short rest, during which he walked about the room, while *E* adjusted the apparatus for the next two trials.

Apparatus. The stimulator (see Fig. 1) consisted of a coil of 20-gauge resistance wire—the heating unit—placed 2.5 cm. from the lower end of a pyrex glass tube, 15 cm. long and 1.2 cm. in diam. The diameter of the lower end of the tube was reduced to 2 mm. by an asbestos plug,[10] which also served to keep the coil at a constant distance from the lower end of the tube. The tube was wrapped in several layers of sheet asbestos, the outermost of which extended around the bottom and top edges of the tube, while the electrical wires leading through the top of the tube were covered with asbestos, and the bare ends were separated by a mica shield. The coil was heated by a current of 6.5 v. and 6.5 amp. The apparatus was held vertically in the clamp of a universal support, and was lowered into position or raised by means of a rack-and-pinion gear.

[10] Various diameters were tried, but smaller diameters did not permit sufficient heat to pass through the openings to arouse pain in the time that was at our disposal, while larger diameters gave areal pains which were not completely adapted even with excessively long stimulation. In one experiment, with a diameter of 1.2 cm., the pain was not completely adapted after 45 min. The pain waxed and waned, frequently seemed to be on the point of disappearing, but never entirely did so. The intensive increases were accompanied by shifts in the locus of the pain. The radiations apparently penetrated deeper and deeper into the skin, and as pain adapted in one part it arose in another. The greater the area stimulated, the less likelihood would there be, then, of all the parts coming to complete adaptation at the same time. That the radiation penetrated deeply into the tissues in the experiment just cited, is shown by the fact that, though never extremely painful, it resulted in a severe burn and in the destruction of the corium. The injury scarred in healing.

Observers. The *O*s were Dr. A. D Glanville (*G*), research fellow in psychology; Mr. W. S. Neff (*N*) and Mr. J. W. Macmillan (*M*), graduate students in psychology; Mr. H. A. Sklarsky (*S*) and Mr. R. Fleischman (*F*), undergraduates; and the authors, who observed only in the preliminary experiments.[17] Of the *O*s in the main experiments, *G*, *N*, and *M* knew the general nature of the problem and were experienced in cutaneous observation, while *S* and *F* had only the information given them in the instructions, and were unpracticed in psychological observation.

Instructions. The following instructions were given at the beginning of every experimental period:

> At a 'Ready,' 'Now' signal, your arm will be stimulated. Describe the temporal course of your experience. Give particular emphasis to the report of the appearance and disappearance of pain. Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the signal, and do not open them until told to do so.

Procedure. The cast in which *O* placed his arm was adjusted before every experiment so that the surface stimulated was horizontal. At "ready" the stimulator was brought to a position directly above this area, and at "now" it was lowered to a point 1 mm. from it.

E recorded *O*'s oral reports, together with the times read off from a stopwatch which was started simultaneously with *E*'s "now."

After a report of the disappearance of pain, stimulation was continued for 20 sec.[18] If the pain reappeared before the 20-sec. interval had elapsed, or if it did not fade out entirely, the stimulator was removed after 4–7 min., depending upon *O*'s susceptibility to injury. Excessive injury was thereby avoided.

Every *O* had a practice series of 5 experiments to familiarize him with the apparatus, task, and routine, and a main series of 25 experiments, conducted at uniform hours twice a week during April and May, 1933.

RESULTS

Normal course of experience. The normal and typical course of experience resulting from stimulation of the skin by radiant heat is illustrated by the following reports.

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[17] We varied, during the preliminary experiments, the size of the area stimulated (see footnote 16), the intensity of the stimulus (in so far as this was affected by the voltage of the heating unit and the distance of the coil from the skin), and the temporal and spatial intervals between stimulations. The constants described under Method and Procedure seemed to give the best results, but none of the conditions could be studied systematically without departing from the object of the present experiment. Every one of the factors could profitably, however, be made the subject of a separate study.

[18] Our procedure differed in this respect from that of Burns and Dallenbach, who did not remove the stimulus "until 20 sec. after *O*'s last positive report of an experience" (*op. cit.*, 115). Pain was followed in our experiments by warmth, which, unlike the residual pressures of their experiments, did not readily reach complete adaptation. Indeed, as we discovered in the preliminary experiments, the skin was blistered and severely injured without the intrudance of pain when attempts were made to prolong stimulation until 20 sec. after the disappearance of warmth—"the last positive report of an experience."

F: [Exp. 13, right arm, area 3]: (2″) warm; (20″) slight heat; (27″) itch; (29″) burning; (35″) itch increasing; (38″) burning itch; (42″) slight pain; (1′23″) steady burning pain; (2′4″) very slight pain but very hot; (2′47″) pain increasing; (2′55″) pain decreasing; (3′8″) no pain but warm; (3′28″, when stimulus was removed) warm.[19] [Retested 10″ after removal of stimulus, pain rearoused 22″ later.]

G: [Exp. 21, right arm, area 1]: (1″) warm; (30″) hot in small area; (37″) heat gone; (49″) heat back; (55″) heat gone; (1′3″) heat back; (1′14″) stingy heat; (2′28″) sting; (2′35″) promise of pain in a small spot, but not yet painful; (3′32″) pain and itch; (4′) pain increasing slightly; (4′7″) pain decreasing; (4′11″) pain increasing again; (4′19″) pain not constant; (4′23″) pain decreasing; (4′33″) not sure whether pain or just intense heat; (4′42″) no pain; (4′55″) itch and pain; (5′) pain increasing; (5′17″) pain decreasing; (5′22″) pain gone; (5′25″) heat almost gone; (5′42″, when stimulus was removed) warm. [Retested at 10″, pain rearoused 18″ later.]

M: [Exp. 4, left arm, area 1]: (1″) warm; (7″) more intense warm; (12″) warmer; (38″) warmer; (50″) hot; (54″) a little pain; (58″) burning pain; (1′1″) stronger pain; (1′7″) intense pain; (1′17″) itchy, burning pain; (1′44″) pain still stronger; (1′47″) tingling burning pain; (2′14″) very strong burning pain; (2′30″) pain a little less; (2′47″) a little less pain; (3′2″) very hot, only a little pain; (3′31″) pain almost gone; (3′46″) pain goes and comes back in little flashes; (4′10″) very hot, but only slightly painful; (4′51″) no pain; (4′58″) a slight pain has returned; (5′21″) no pain, just heat; (5′41″, when stimulus was removed) heat. [Retested at 10″, pain experienced 30″ later.]

N: [Exp. 5, left arm, area 1]: (1″) warm; (10″) intense warmth; (18″) more intense; (25″) warmth with a fringe of pain; (29″) sharp pain; (36″) pain sharply localized; (42″) flash of cold; (55″) sharp pain; (1′17″) heat with a fringe of pain; (1′37″) diffuse, intense, burning heat; (2′8″) fluctuating pain; (2′30″) hot; (2′48″) diffuse heat of lower intensity; (3′) just heat, no pain; (3′20″, when stimulus was removed) intense warmth. [Retested at 10″, pain experienced 20″ later.]

S: [Exp. 24, right arm, area 4]: (1″) warm; (33″) warmth with slight pain; (35″) increase in pain, warmth slight; (48″) pain increased; (1′) pain and heat; (1′8″) pain decreased; (1′15″) pain increased; (1′57″) pain decreasing, no heat, only warmth; (2′16″) no pain, slight warmth; (2′33″) very slight pain; (2′59″) very slight pain and warmth; (4′25″) no pain, just warmth; (4′45″, when stimulus was removed) slight warmth. [Retested at 10″, pain experienced 15″ later.]

The experiences aroused by radiant heat begin with warmth. This quality is reported first by all *O*s in all the experiments. From this point on, experience deviates but slightly in the different experiments from a general course passing through intensive gradations of warmth to heat, and through similar gradations of heat to weak pain; the pain then grows in intensity, remains constant at a high degree for a short time, and gradually declines and disappears. The decline of pain is accompanied by the reappearance of heat and of warmth, the latter persisting after pain has disappeared, and frequently even after the removal of the stimulus.

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[19] The numbers in parentheses in this and the following reports show the time that has elapsed between the beginning of the experiment and the report they accompany.

The typical course of the experience may be divided into three phases: (1) a fore-phase, beginning with the application of the stimulus and ending with the arousal of pain; (2) a mid-phase, running from the appearance of pain, through its growth, to the beginning of its decline; and (3) an end-phase, which begins with the decline of pain and ends with the interruption of the experiment.

Arousal of pain. Pain did not appear early in any of the experiments. The average time required for its arousal varied, as Table I shows, from 40.3 ± 16.5 sec. (for *S*) to 59.4 ± 41.7 sec. (for *G*). The separate times ranged from 5 to 212 sec.

TABLE I

SHOWING THE AVERAGE TIME IN SEC. REQUIRED FOR THE AROUSAL OF PAIN, THE M.V., AND THE RANGE OF DISPERSION

O	Av.	m.v.	Range
F	51.1	26.8	14–151
G	59.4	41.7	6–212
M	44.1	19.6	7–123
N	41.9	23.6	5–161
S	40.3	16.5	5–75

Not only was pain slow in coming, but it was always mild and weak on its first appearance, as the following characterizations of the first pains, in every one of the experiments with every *O*, clearly show.

F: itch (10); slight itch (8); ticklish burn (2); slight burn (1); slight ticklish pain (1); slight pain (1); slight burning pain (1); very slight itch (1).[20]

G: slight pain (6); mild pain (3); small pain (3); stingy pain (2); hint of pain (1); sting (1); small point of pain (1); slightly stingy (1); itch (1); very slight pain (1); and 5 cases in which the first pain was not specifically characterized, though the next and subsequent reports of pain show that the first was unintense. For example, consider *G*'s reports in Exp. 15: pain appeared at 35 sec. when it was described merely as "pain;" at 40 sec., he reported that it was "increasing;" at 45 sec., "still increasing;" at 49 sec., "increasing in intensity;" at 59 sec., "stabbing pain;" at 65 sec., "still increasing;" etc. This progression can only mean that the first pain was mild or weak.

M: slight pain (13); very slight pain (7); a little pain (1); a flash of pain (1); and 3 cases in which the first pain was not characterized, but was shown by the next and subsequent reports to be weak.

N: slight pain (12); very slight pain (3); itch (1); a kind of pain (1); a point of pain (1); tingle of pain (1); fringe of pain (1); and 5 cases not characterized but shown by subsequent reports to be weak.

S: slight pain (10); very slight pain (7); extremely slight pain (1); flash of pain (1); and 6 cases not characterized, but shown by the next and following reports to be mild or weak.

[20] The numbers in parentheses indicate the frequency of use.

These results clearly show that our stimulus was not of a kind to produce a *plötzlich einbrechender starker Schmerz*. We appear, therefore, to have met successfully the conditions laid down by Goldscheider as necessary for the perception of the *unterschmerzliche Empfindungen*.

Pressury experiences. Despite the fact that our conditions were apparently optimal for the observation of pressury experiences during the period before the arousal of pain—the period during which, according to Goldscheider's theory, the pressures should be both numerous and insistent—our *O*s reported pressury experiences in only 8 of the 125 experiments. Of these exceptions (2 with *F*, 4 with *N*, and 2 with *S*—*G* and *M* did not report pressures antecedent to pain in a single experiment), none indicates that pain arose from or because of a summation of the pressury experience.

F reported "tickle" before pain in Exp. 2 and 4; *N* reported a "pressury sensation" in Exp. 1, "tickle" in Exp. 7 and 20, and "tingle" in Exp. 15; and *S* reported a "dull pressure" in Exp. 13, and a "slight pressure or contact" in Exp. 17. Their reports in these experiments, from the stimulation to the arousal of the first pain, were as follows.

F: [Exp. 2, left arm, area 3]: (3″) warm; (15″) hot; (25″) hot and tickle; (36″) slight ticklish pain; (43″) increasing pain. [Exp. 4, right arm, area 4]: (10″) warm; (28″) hot and tickle; (38″) ticklish burn; (43″) same plus pain.

N: [Exp. 1, left arm, area 4]: (5″) warm; (50″) peculiar pressury sensation; (1′10″) kind of pain as though skin were being drawn up, a pulling sensation; (1′43″) a sharp pain is now merged with this. [Exp. 7, left arm, area 2]: (3″) warm; (45″) tickle; (47″) itch; (49″) pain. [Exp. 15, left arm, area 3]: (3″) warm; (41″) warm increasing; (1′12″ tingle of pain; (1′14″) burning pain. [Exp. 20, right arm, area 1]: (1″) warm; (20″) tickle; (25″) pain; (29″) pain increasing; (31″) increasing; (38″) still increasing; (40″) sharp pain.

S: [Exp. 13, left arm, area 4]: (1″) warm; (3″) heat; (18″) dull pressure; (44″) slight pain. [Exp. 17, left arm, area 4]: (3″) warm; (28″) slight pressure or contact; (48″) slight pain.

"Tickle," "draw," "tingle," and "contact" are beyond doubt pressury experiences, but there is no evidence for the belief that they are related to pain, or that pain is in any way dependent upon them. Pain was aroused in 117 of the 125 experiments without the intrusion of pressure. Since pure, pressureless pains occurred so frequently (in 94% of our experiments), and since our results corroborate those obtained by Von Frey, we feel justified in concluding with him that Goldscheider's summation theory of pain is untenable.

This conclusion is supported by the results of the second and third phases of the experiments. During the second, the pain-phase, 2 of our

Os reported pressures in 12 experiments (9 by *N*, and 3 by *S*) after the appearance of pain or together with it. These instances are to be sure not numerous; but if pain is, as Goldscheider claims, the summation of pressures, we should not expect the sum to appear before the parts, nor the sum and the parts to co-exist. Such reports, as are illustrated by the following excerpts, are inconsistent, therefore, with Goldscheider's theory.

N: [Exp. 6, left arm, area 4]: (2′20″) a little pain mixed with pressure and warmth. [Exp. 9, left arm, area 3]: (38″) very sharp pain mixed up with burning heat; (64″) addition of drawing pressure quality. [Exp. 15, left arm, area 3]: (1′12″) tingling pain; (1′14″) burning pain with lots of heat in it; (1′40″) now pressure in it; [Exp. 19, left arm, area 3]; (7″) slight warm, (15″) pain; (20″) pain increasing; (28″) intense pain; (35″) increasing; (53″) very sharp pain; (1′28″) very lively pain; (1′42″) a lot of drawing pressure in it.

S: [Exp. 11, right arm, area 3]: (1″) warm; (11″) pain and warmth; (35″) pain and heat; (42″) pressure added; (1′5″) still pressure, pain, and heat. [Exp. 12, left arm, area 2]: (3″) warm; (32″) very slight pain; (45″) pain more definite but still slight; (1′5″) pressure came in; (1′20″) pressure, pain, and heat. [Exp. 22, left arm, area 3]: (4″) warm; (55″) slight pain; (60″) pain increasing; (1′8″) the slight pain increasing; (1′18″) pain remaining constant; (1′30″) I think there is pressure with it now.

During the third, the end- or adaptation-phase of the experiments, pressury experiences were reported 11 times by 3 *Os* (4 by *G*, 5 by *N*, and 2 by *S*); but only 5 of these (1 by *G*, 3 by *N*, and 1 by *S*) were reported after the disappearance of pain. If Goldscheider's theory of pain were correct, we should expect pain always to be followed by pressury experiences. As we shall return to this point presently, it will suffice here merely to note the inconsistency between fact and theory.

The pressury experiences noted in these experiments offer no support for the summation theory of pain. Whence do the pressures come, and how and by what are they aroused? We have no knowledge which will enable us definitely to answer these questions, but we believe that they are incident to heat. Not only were the pressury attributions chiefly used by our *Os* in connection with "hot," "heat," and "burn," but the analyses of heat that have been made reveal that it is itself "an astringent quality that lies in the qualitative continuum between pressure and pain."[21] Moreover, our stimulus may not have been altogether pressure-free. "It is possible," as Dimmick has pointed out, "that the radiant heat causes the

[21] N. C. Burnett and K. M. Dallenbach, The experience of heat, this JOURNAL, 38, 1927, 422. Cf. also F. L. Dimmick, On the localization of pure warmth sensations, *ibid.*, 26, 1915, 145; F. Cutolo, A preliminary study of the psychology of heat, *ibid.*, 29, 1918, 445 f.; J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *ibid.*, 31, 1920, 311; S. C. Ferrall and K. M. Dallenbach, The analysis and synthesis of burning heat, *ibid.*, 42, 1930, 72-82.

skin to contract, and thus produces a direct stimulation of the organs of pressure."[22]

Adaptation of pain. According to the criterion of adaptation which we accepted for the purposes of this study—namely, the disappearance of pain for an interval of 20 sec.—complete adaptation occurred, as Table II shows, in 76 (60%) of our experiments—in 19 for *F*, 10 for *G*, 15 for *M*, 11 for *N*, and 21 for *S*. We chose this rather rigorous criterion (1) so as to bring our results into line with those of Burns and Dallenbach, who employed a similar criterion; and (2) so as to obviate all questions regarding accuracy of observation, the reason for their original choice, since disappearances for 20 sec. can hardly be explained away by imagination or distraction.

TABLE II

SHOWING THE NUMBER OF EXPERIMENTS IN WHICH PAIN DISAPPEARED AND THE NUMBER OF TIMES PAIN DISAPPEARED FOR 20 SEC. OR FOR SHORTER INTERVALS

O	No. experiments		Duration		Total no. cases
	disappeared	did not	20 sec.	less than 20 sec.	
F	22 (88%)	3 (12%)	19	26	45
G	21 (84%)	4 (16%)	10	25	35
M	23 (92%)	2 (8%	15	29	44
N	17 (68%)	8 (32%)	11	32	43
S	21 (84%)	4 (16%)	21	4	25
Total	104 (83%)	21 (17%)	76	116	192

Had we chosen a less exacting criterion, more cases of adaptation would have been recorded; for pain disappeared, as Table II also shows, 116 times for intervals of less than 20 sec. (26 for *F*, 25 for *G*, 29 for *M*, 32 for *N*, and 4 for *S*). The duration of these cases, examples of which are given in the excerpts below, varied from 2-19 sec., the average duration being about 10 sec.

F: [Exp. 20, right arm, area 1]: (2′54″) pain; (3′10″) no pain; (3′29″) pain; (3′50″) no pain; (4′5″) pain; (4′27″) pain decreasing; (4′52″) no pain; (5′6″) pain; (5′25″) no pain; (5′45″ stimulus removed). [Retested at 5″, pain reëxperienced 12″ later.]

G: [Exp. 4, right arm, area 2]: (3′36″) pain increasing; (3′45″) just barely there; (3′50″) it's gone; (3′53″) back again; (4′7″) gone; (4′11″) back; (4′15″) it surges up and down, appears and disappears; (5′5″) slight increase, fairly intense; (5′15″) gradually fading; (5′23″) increasing; (5′33″) going gradually; (5′40″) almost gone; (5′52″) back again; (6′1″) fading; (6′5″) pain gone; (6′25″ stimulus removed). [Retested at 10″, pain reëxperienced 15″ later.]

[22] F. L. Dimmick, On the localization of pure warmth sensations, this JOURNAL, 26, 1915, 145.

M: [Exp. 18, left arm, area 1]: (2′) very strong pain; (2′8″) less pain; (2′14″) less pain; (2′22″) dull pain; (2′28″) still decreasing; (2′45″) just slight pain; (2′55″) increasing again; (3′2″) decreasing; (3′9″) pain gone; (3′13″) back, very slight; (3′25″) pain gone; (3′35″) slight pain, comes and goes; (3′48″) pain gone; (4′7″) slight pain; (4′15″) increasing; (4′20″) decreasing; (4′26″) pain increasing; (4′36″) still increasing; (4′45″) pain intense; (5′ stimulus removed).

N: [Exp. 23, right arm, area 4]: (4′2″) pain and heat; (4′10″) very slight thrilling pain; (4′22″) pain gone; (4′33″) back; (5′3″) intermittent slight pain; (5′47″) gone; (5′58″) pain; (6′3″) gone; (6′10″) slight pain; (6′16″) gone; (6′26″) pain again; (6′37″) gone; (6′50″) slight needle of pain that comes and goes; (7′15″) pain fading, alternates with heat; (8′5″) steady and increasing pain; (8′30″) definite pain; (8′40″) increasing; (9′ stimulus removed).

S: [Exp. 19, right arm, area 4]: (1′15″) pain increasing; (1′57″) pain decreasing, no heat only warmth; (2′16″) no pain, slight warmth; (2′33″) very slight pain and warmth; (4′25″) no pain, just warmth; (4′45″ stimulus removed) warmth. [Retested at 10″, pain reëxperienced 15″ later.]

Our experiments, it will be recalled, were not of a fixed length. The stimulus was continued until a 20-sec. period of adaptation was obtained, or until it appeared that the skin was about to be grievously injured. Whether disappearances of 20 sec. would have been obtained in all experiments if stimulation had been prolonged, we seriously doubt, since the

TABLE III

SHOWING FOR THE EXPERIMENTS IN WHICH COMPLETE ADAPTATION OF 20 SEC. OCCURRED, THE AVERAGE TIME IN SEC. REQUIRED FOR THE ADAPTATION OF PAIN AFTER ITS FIRST AROUSAL, THE M.V., AND THE RANGE OF DISPERSION

O	No. cases	Av.	m.v.	Range
F	19	46.3	36.9	8–146
G	10	104.1	83.0	2–241
M	15	135.9	63.1	38–319
N	11	122.5	56.1	10–212
S	21	97.0	54.3	1–187

effect of radiant heat, unlike that of punctiform stimuli, is not confined to a definite point. The heat rays, penetrating deeper and deeper into the tissues and dispersing more and more widely through the skin, must stimulate an increasing number of nerve endings. Thus it could be only by chance that an interval of 20 sec. would elapse between the adaptation of pain at one nerve ending and its arousal at another. The experiments in which the disappearance of pain was not reported may, therefore, be interpreted as instances in which new nerve endings were stimulated before adaptation had run its course. Regarded in this light, the number and percentage of cases of complete adaptation we obtained is extremely high. In any case, it is high enough to warrant the conclusion that the pain of radiant heat is adaptable.

The time from the first arousal of pain to its complete adaptation varies considerably, as Table III shows, from 8-146 sec. for *F*; 2-241 sec. for

G; 38-319 sec. for *M*; 10-212 sec. for *N*; and 1-187 sec. for *S*. The average adaptation-times show large individual differences, varying from 46.3 sec. (for *F*) to 135.9 sec. (for *M*). These deviations may be due to differences in the resistance of *O*'s skin to radiant heat, but the size of the m.v. and range of dispersion point rather to chance differences in depth of stimulation.

It must be remembered that complete adaptation is but one of several causes of the disappearance of pain, other possible causes are nerve block, destruction of the nerve ending, or distraction. As we have assumed above, distraction was eliminated by the adoption of the 20-sec. criterion, but there still remains the possibility of nerve block or destruction of the nerve endings—particularly the latter, as in spite of utmost precautions, the skin was frequently blistered and burned. In order to test these possibilities, we restimulated the points that had yielded complete adaptation (as the excerpts reported on pp. 234, 238f. show) at intervals ranging from 5 to 20 sec. after the conclusion of the principal experiment. *In every case pain was reëxperienced;* thus, the 20-sec. disappearances reported in this study are true cases of adaptation.

As Table IV shows, the time from restimulation to the rearousal of pain varied considerably, ranging from 16 sec. (for *M*) to 43.3 sec. (for *G*). A comparison of the values in this tables with those in Table I—which gives the data for the first arousal of pain—reveals that the stimulation-times of the rearousals are on the average from 9.7 sec. (for *N*) to 28.1 sec. (for *M*) shorter than the first arousals (see last column, Table IV). These results are due, we believe, to a "softening effect" of the first stimulations. We venture this suggestion, first, because the correlations between the times of arousal and rearousal are positive, though low, for all our *O*s. As computed by Spearman's Foot-Rule, $R = 1 - [6\Sigma g/(n^2 - 1)]$, the coefficients are 0.21 for *F*, 0.04 for *G*, 0.20 for *M*, 0.29

TABLE IV

SHOWING FOR THE EXPERIMENTS IN WHICH COMPLETE ADAPTATION OF 20 SEC. WAS OBTAINED, THE AVERAGE TIME IN SEC. REQUIRED FOR THE REAROUSAL, THE M.V., AND RANGE OF DISPERSION, TOGETHER WITH THE DIFFERENCE BETWEEN THE TIME OF AROUSAL AND REAROUSAL

O	No. cases	Av.	m.v.	Range	Difference between arousal and rearousal
F	19	28.3	15.2	5–96	22.8
G	10	43.3	23.7	15–113	16.1
M	15	16.0	3.0	7–47	28.1
N	11	32.2	15.4	12–70	9.7
S	21	27.8	14.7	5–90	12.5

for *N*, and 0.29 for *S*. Secondly, as the m.v. and the range of dispersion show, the lower average time of the rearousals was obtained chiefly through the shortening of the upper range of dispersion. By comparing Tables I and IV we find that the upper range of the times of rearousal is considerably shorter for 4 *O*s, while the lower range is approximately unchanged, being identical for *M* and *S*, slightly larger for *G* and *N*, and slightly smaller for *F*. Since the "softening effect" would be proportional to the length of the first stimulation, these results—a low positive correlation, a smaller m.v., and a shortening of the upper range of dispersion —are the very results we should expect. Long primary stimulations would have great effect, short ones hardly any at all.

The normal and most frequent course of adaptation was, as we observed above, a gradual decline in intensity and a disappearance of pain. The decline was accompanied by the reëmergence of heat and of warmth, the latter persisting after the disappearance of pain and usually until the removal of the stimulus.[23] Pressury experiences, which should according to Goldscheider inevitably follow pain in the course of adaptation, were reported in but 5 experiments. These 5 cases—as well as all the other instances of complete adaptation—were, however, closed by reports of warmth. The pressures reported were mild and weak, as were also many of the warmths. Our experiments, therefore, are not subject to the criticism that the intensity of the end-phase masked or obscured the *"unterschmerzliche Empfindungen."* Had these experiences been present, they would have been reported more frequently by our *O*s. That they were reported in only 5 out of 76 experiments showing complete adaptation, while warmth was reported in all of them, is further evidence, we believe, for the untenability of Goldscheider's pressure-theory of pain.

The results of this study might lend support to a new theory that pain is a summation of *unterschmerzliche Wärmeempfindungen;* but we prefer the conclusion that the accompaniments of pain are functions of the type of stimulus employed in its arousal. When a pressure-pain stimulus is used, residual pressure-effects are noted. When a warmth-heat-pain stimulus is used, residual warmth-effects are noted. It is our belief, further-

[23] These results, it is interesting to note, stand in direct opposition to those reported by Heinbecker, Bishop, and O'Leary (Analysis of sensation in terms of the nerve impulse, *Arch. Neur. & Psychiat.*, 31, 1934, 34-53). These authors state that "burning pain from a hot object lost its quality of being hot before that of being painful, and finally only touch was recognized" (p. 42). They do not, however, tell us anything about the conditions under which their observation was made. Lacking these essential items of knowledge, we can do no more than to note the discrepancy, and to point out that their results fall in line with Goldscheider's. We should also point out that their use of psychological terms is both naïve and ambiguous.

more, that if a cold-pain stimulus were employed, residual cold-effects would be noted. However that may be, sufficient facts are at hand to justify us in concluding with Von Frey, that pain is a separate modality of experience which may, under proper conditions, be completely adapted without undergoing any qualitative changes.

The conclusion that pain is a separate modality with nerve fibers of its own is corroborated by the results of recent experiments in the field of neurophysiology. Gasser and Erlanger,[24] and more recently Waterston,[25] have demonstrated that pain is subserved by specific nerve fibers. Though these authors are not in complete agreement regarding the details of the correlation between fiber and function, they accord in the view that separate nerve fibers "convey the sensations of pain."

Summary

Under the conditions of this experiment, which successfully meet Goldscheider's strictures against the use of radiant heat as a stimulus of pain, we find:

(1) That pure, pressureless pains are aroused by radiant heat.

(2) That these pains disappear without undergoing qualitative changes.

(3) That the disappearances, as the restimulation and rearousal showed, are real cases of sensory adaptation.

(4) That warmth is an invariable accompaniment in the fore- and end-phase of the pain aroused by radiant heat.

(5) That pressury experiences are not inherent in pain as they were neither the invariable nor constant accompaniment of the pain aroused by radiant heat.

In view of these results we feel justified in concluding:

(1) That Goldscheider's theory of pain is untenable.

(2) That the accompaniments of pain—pressure, as in the earlier experiments, and warmth, as in this experiment—are functions of the type of stimulus employed. A warmth-heat-pain stimulus has a residual warmth effect when pain is adapted. A pressure-pain stimulus has similarly a pressure effect.

(3) That pain is, as Von Frey contends, a separate modality of experience which may, under proper conditions, be completely adapted without undergoing any qualitative changes.

[24] H. S. Gasser and Joseph Erlanger, The rôle of fiber size in the establishment of a nerve block by pressure or cocaine, *Amer. J. Physiol.*, 88, 1929, 581-591.

[25] David Waterston, Observations on sensation: The sensory functions of the skin for touch and pain, *J. Physiol.*, 77, 1933, 251-257.

AN EMPIRICAL COMPARISON OF AUDITION, VISION, AND TOUCH IN THE DISCRIMINATION OF SHORT INTERVALS OF TIME

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The purpose of this research was to make an investigation of time discrimination in the three main sense modalities: vision, audition, and touch. Much research has been done in the field of audition, and some in vision and touch, but practically no work has been done toward a comparative study of the three sense modalities. Miss Gridley,[1] using the Seashore phonograph record for the measurement of the "sense of time," instrumentally communicated to the *O* by the teletactor, compared hearing and touch and found that her *O*s made 5% higher scores in the auditory situation than in the tactual. Most research has followed the plan of varying the length of the intervals studied under constant conditions. In this research we have decided to use one length of interval (1 sec.), and to study it under varying conditions, using different techniques, and the three sense modalities.

APPARATUS

Observer's room. All the observations were made in a small room approximately 6 x 9 x 9 ft. The room was well ventilated and the temperature kept about 68°. The room was painted pale green and experiments were done under reduced illumination. The window shade was raised 18″ from the sill; when experiments were done at night, a 5 Watt lamp furnished illumination.

The stimuli were of 3 main types, auditory, visual, and tactual. For the source of auditory stimuli, a Western Electric receiver unit (56 ohm impedance) was mounted on the wall about 4′ in front of *O*. The regular stimuli were short "clicks" which were quite loud.[2] By introducing resistance into the line, the intensity could be reduced to the limit of audibility. A 4-watt commercial neon lamp (110 volt) supplied stimuli for vision. The neon lamp was inside a 4-in. tube, the open end of which was covered with several layers of tissue paper. This served to dim the visual stimuli and prevent definite fixation such as on the filament of the lamp. The stimuli

* Accepted for publication September 6, 1932. The author is indebted to Professor R. H. Gault of Northwestern University for proposing this problem and for many helpful suggestions during its course.

[1] Pearl Gridley, The discrimination of short intervals of time by finger-tip and by ear, this JOURNAL, 44, 1932, 18-43.

[2] A rough estimate of the intensity can be made by an experiment in which the receiver was placed outside and away from reflecting walls. It was audible to a person with normal hearing within a distance of 80 ft.

appeared as an illuminated surface. A teletactor unit furnished stimuli for touch.[2] It made some sound which of course had to be eliminated. Stopping *O*s ears made him unconscious of any auditory stimuli, but there was still the possibility of subliminal stimuli which might influence his judgment. Consequently we secured two concrete boxes, one inside the other. The outside box (32 in. cube) contain an inside box (24 in. cube) which rested on springs and was lined with 2 in. of cotton. The teletactor unit was suspended on springs from the top of the inside box. The wires through which the teletactor unit was activated were laid in the concrete before it was

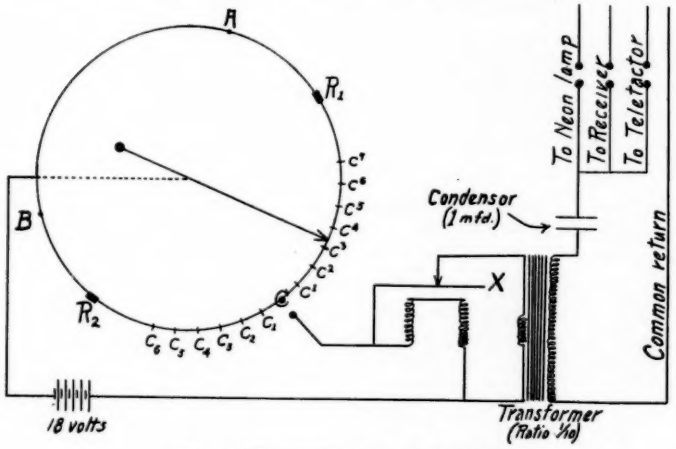

FIG. 1. WIRING DIAGRAM OF APPARATUS

set. Between the two boxes and between the teletactor unit and the inside boxes the wires were carefully coiled so as to prevent the conduction of any sound out over the wires. The *O*, in order to feel the teletactor, thrust his arm through a hole 3½ in. in diam., which was padded on both sides of each box by a collar of rubber sponge. This precluded all possibilities of auditory clues from the teletactor, as evidenced by a test of subliminal stimulation which will be reported later.

The receiver unit, neon lamp, and the teletactor unit were all activated by short electrical impulses which could be spaced at any desirable interval between 700 and 1300 σ by *E* in another part of the laboratory. In order to reduce the duration of the stimulus to a minimum an automatic circuit breaker was inserted between the distributing board and *O*. Thus as the batteries were connected, the magnet was energized and contact (Fig. 1) was immediately broken. Furthermore, a step-up transformer (ratio 1-10) increased the potential, and also prevented any continuous flow of current. A one microfarad condenser in the line eliminated everything but the peak of the wave. This impulse gave very clear sharply defined stimuli, the intensity of which could be varied by means of a rheostat.

[2] The teletactor unit is a powerful low impedance magnetic vibrator which is used by Dr. Gault in connection with the tactual interpretation of speech.

As a variation, the apparatus was designed so that continuous stimulation could be given and the electric impulses served to break the circuit for an instant. For example, in the visual field, the neon lamp could be lighted steadily, and the intervals of time marked off by very brief flickers. This was accomplished by connecting a 110 volt A.C. line, through a 25 watt lamp to the neon lamp. The electric impulse distributor was connected across the line, so that it served to short-circuit the neon lamp for an instant, thus causing a flicker and marking off the desired interval of time.

A variation in tactual stimuli was accomplished by energizing the teletactor through the teletactor amplifier. As this is an A.C. amplifier, there is a slight steady hum in the teletactor unit. The impulses stood out plainly on this background of vibration. This variation in tactual stimuli was made with the thought that possibly the results from the 3 sense modalities might be more nearly comparable if a background of vibrations were provided in the tactual sphere, since in both the auditory and visual fields we had a background of sound and light respectively.

All instructions were given to *O* in writing. This was accomplished by a board 9 in. by 14 in. containing 10 small electric lamps on the left margin beside which instruction cards were placed. *E* indicated by a light the particular set of instructions that he wished *O* to follow. *O* held a small block on his lap which contained two keys by which he made all his responses. Telephone connection was available if needed.

Experimenter's room. *E* worked in a small room in another part of the laboratory. There were three walls between *O*'s room and *E*'s so that the operation of *E*'s apparatus was not audible to *O*.

The chief equipment was an electric impulse distributor which marked off intervals of time (the term 'interval of time' is used here to designate the period between two stimuli) varying from 700 to 1300 σ in steps of 20 σ. The distributor consisted of a brush 21 in. long which was rotated by a synchronous motor (1800 R.P.M.) geared down to 20 R.P.M. Thus the brush made a complete revolution every 3 sec. The circumference being about 132 in., the brush moved 0.0421 in. or approximately 1/24 in. in 1 σ. The spacing of the posts was accurate to 1/32 in. so that it may be safely said that the instrument was accurate to ± 1 σ. The only possibility of error beside the placing of the contact posts was in the speed of the motor. Being a synchronous motor, its speed depended upon frequency of the current which, during the past year, has not varied more than 0.08%. Contact posts were placed at various positions on the circumference and each one was connected to a switch by which that particular post could be connected to the receiving apparatus. The brush was grounded to the frame of the motor which was in turn grounded. Since current was supplied by a central battery system in which the negative pole was grounded, this served as the negative side of the line. The receiving apparatus was connected to the positive side of the battery and to the electric impulse distributor. In the first part of the experiment, which employed the method of Right and Wrong Cases, the *O*s were asked to compare 2 intervals marked off by 3 stimuli (clicks, flashes, and punches as the case might be). That is, the question for *O* to decide was this: "Was the interval between the first and second stimulus longer or shorter than the interval between the second and third?"

The standard interval in this experiment was 1 sec. and the other interval varied from 1 to 1.3 sec. In this part of the experiment the standard interval was first half

of the time and last the other half of the time. To accomplish this, a double-throw switch connected either R_1 or R_2 (Fig. 1) to a relay switch which would make or break the circuit to the observation room. For example, if the switch was thrown connecting R_1 to the relay, the relay was tripped when the brush touched it, thus connecting the distributor to the observation room. The brush then touched contact A which sent the first impulse, then, a second later, contact B, and finally contact C superscript (the particular one depending upon the length of the interval desired). When the brush came to R_1 again the line to the observation room was broken and the brush continued to rotate without sending any impulses until it struck R_1 again. Thus there was a little more than 3 sec. between each series of impulses. Now if *E* desired the standard interval to be last instead of first he could merely throw the double-throw switch, making R_2 energize the relay. This also connected the series of posts labeled C subscript to the line. By another series of switches *E* determined which of the C subscript posts was to be connected to the line. When the brush struck R_2, the relay connected the distributor to the observation apparatus. Then the C post which was connected gave the first impulse and A the second—an interval of more than 1 sec. The third impulse came from B, making the second interval the standard. As the brush struck R_2, the line was disconnected until the brush struck it the third time, or *E* turned the switch to R_1.

O responded by pressing one of two keys, depending on which interval he estimated to be the longer. These keys were wired in series with *E*'s switch which determined which interval was longer, so that if *O* pressed the proper key, it was recorded on an electric impulse counter. Another impulse counter recorded the total number of trials given. In the latter part of the research the electric-impulse counter refused to work accurately, so two signal lights were substituted, from which *E* recorded *O*'s responses.

In the part of this research in which the method of Average Error was used, some additional apparatus was necessary. Here *O* was presented with the two stimuli marking off the standard interval and was asked to produce the third by pressing a key at the proper time. The key operated a relay which produced a stimulus (click, flash, or punch) exactly like the regular stimulus. When *O* pressed the key, a contact was made and an impulse just like the impulse which came from the distributing board was sent into the line.

The key circuit was also connected to a series of small lamps which was connected to the various contact posts on the distributing board. *O* should respond as the brush passed contact post C. By watching the lights, *E* could tell when *O* actually pressed the key, and read directly the error made.

Preliminary Experiments

Auditory stimuli. In the description of the apparatus the exact circuit and electrical measurements have been described so that the experimental conditions can be reproduced exactly. As a means of explaining the approximate intensity of the click, we connected the receiver to an extension cord, took it outside away from the building, and measured the greatest distance a person with normal hearing could be from the receiver and still hear the clicks. This experiment was done at midnight when all was quiet. It was found audible up to a distance of 80 ft. A less intense click was produced by inserting 2000 ohms resistance into the line. This could be heard at a distance of 35 ft. Both clicks were clearly above the threshold in the

experimental situation as the receiver was approximately 4 feet from *O*. A study using 5 *O*s was made to determine the effect of the different intensities on accuracy of discrimination.

Three different levels of discrimination were examined: one set of intervals in which the difference in length was well above *O*'s differential threshold, one set at the threshold, and a third set below the threshold. *O* was given the following instructions:

> You will hear 3 clicks marking off 2 intervals of time. One interval will always be 1 sec., and the other will be a little longer. If the second interval is longer, press the key marked 'L,' if shorter, press the key marked 'S.'

Then he was presented with sets of clicks of the maximum intensity, followed by 10 of the less intensity, and so on until he had made 120 judgments. It was found that the difference in intensity made no significant difference in the result: 2 *O*s showed better discrimination with the loud click, 1 showed no difference due to intensity, while the other 2 showed keener discrimination when the less intense clicks were used.

Visual stimuli. The source of the visual stimuli, as we have explained, was a small neon tube. We found that the flash from the neon tube was not always from the same part of the tube. Furthermore, it left a definite after-image. Therefore we mounted the tube inside a tissue paper covered frame 4 in. in diam. Then the stimulus appeared as a short faint glow of the tissue paper surface.

The question occurred to us as to whether a flash was the best type of visual stimulus, since our problem was the study of time discrimination in response to visual stimuli in general rather than to flashes; flashes of light being only one form of many possible visual stimuli. As an alternative visual stimulus, we connected the neon lamp so that it was constantly illuminated and the intervals were marked off by a just-discernible flicker. Thresholds were computed by three psychophysical methods; namely, Right and Wrong Cases, Average Errors, and Just Noticeable Differences. Eight *O*s served for this experiment and the differential threshold values showed no significant difference in the two forms of visual stimuli. Because it was more convenient, the flash was used throughout this research.

The next problem was to decide whether in the visual part of the experiment it was best to work in the dark or the light. Both lag and persistence of sensation are involved. Persistence of sensation is at a minimum with low intensities and light adaptation. However, lag is probably tending toward a maximum with light adaptation. The problem is extremely complicated and, according to Ferree,[4] depends on the nature and composition of the light, intensity, light or dark adaptation, and the individual *O*. We did not attempt to consider all the factors involved, being content to reduce the intensity to a minimum and use the conditions which gave the finest differential sensitivity. Eight *O*s served for this experiment and were given the following instructions:

> You will see 3 flashes of light marking off 2 intervals of time. One interval will always be 1 sec., and the other will be a little longer. If the second interval is longer, press the key marked 'L'; if shorter, press the key marked 'S.'

As in sound, 3 degrees of difficulty were used: one set of intervals in which the difference was above *O*'s differential threshold, one in which the difference was ap-

[4] C. E. Ferree in a personal letter.

proximately at the threshold, and one in which it was below the threshold. The *O*s would be allowed to make about 20 judgments under light adaptation and then the room was darkened. While adaptation was taking place, *O* worked on another experiment. After about 5 min., we returned to the light-dark adaptation study, and gave *O* 20 trials under dark adaptation. This process was continued, at each change allowing time for adaptation, until each *O* had made 120 judgments. The same experiment was performed using the flicker instead of the flash. The results showed that for the flashes, discriminations were a trifle keener under light adaptation than under dark adaptation, but for the flicker, the difference was negligible. Consequently, all the experiments were done under light adaptation, although the illumination was slightly subdued.

Tactual stimuli. The tactual stimuli may be considered for all practical purposes to consist of a single movement of the diaphragm of the teletactor, because the chokes and condensors in the line, as described in the account of the apparatus, eliminated everything but the peak of the wave. At first we thought that possibly the punches ("punches" is adopted to refer to tactual stimuli felt over the teletactor) should be presented in a background of vibrations in order that the tactual situation might be more nearly comparable to the visual and auditory situation in which there was naturally a background of light and sound respectively. To supply this background the teletactor receiver was activated through the amplifier which gave a steady low-pitched hum. In this arrangement, the punches stood out clearly from the background of vibration. However, after trying it with 3 *O*s we concluded that the type of tactual stimuli made no apparent difference in accuracy of discrimination. Consequently, for the remainder of the experiment, we used only the discrete impulse hook-up, as it was more convenient.

One more investigation was necessary in connection with the tactual part of the experiment; namely, to ascertain whether the *O*s were receiving any auditory clue. Although they were not conscious of any sound coming from the soundproof box containing the teletactor, it was conceivable that the *O*s might be influenced by some subliminal auditory stimuli. To test this proposition, we set up a simple experiment. Four *O*s served in this test, and were given the following instructions:

> Put your arm into the box as you do in the experiment, but do not touch the diaphragm of the teletactor. Every 5 sec. the light will flash. Half of the flashes will be accompanied by stimuli over the teletactor. You are to guess each time the light flashes whether or not the teletactor has been activated.

None of the 4 *O*s was able to guess correctly more times than chance would allow. Therefore we were convinced that no subliminal auditory clues were operative in the tactual situation.

TECHNIQUE

The first attempt to measure differential sensitivity was by the Method of Right and Wrong Cases. The *O*s were presented with 3 stimuli marking off 2 intervals of time, and each one was asked to decide whether the second interval was longer or shorter than the first. The data to be presented later are based on approximately 14,000 judgments by this method.

Another approach was afforded by the Method of Just Noticeable Dif-

ferences. Two intervals of time in which the second was considerably longer were presented to *O*, and the pair was repeated, each time decreasing the length of the second interval until the *O* reported they were equal. The second was further decreased until *O* reported a noticeable difference. The same process was repeated with an ascending series of values, until sufficient judgments had been made to compute a threshold. Over 1000 judgments were collected by this method.

The Method of Reproduction (Average Error) was employed in a setup in which only the first intervals were determined by the impulse distributor, and *O* reproduced the second interval. The apparatus was so designed that the stimuli which *O* produced were quantitatively and qualitatively exactly like the regular stimuli. The motor response required of *O* to reproduce the interval was the depression of a key. By means of a chronoscope, *E* could read directly the error made by *O*.

Right and Wrong Cases. The first problem in connection with this technique was the determination of the effect of position of the interval in the pair being judged. In preliminary experiments, we found that out of 2385 judgments, 13% more correct judgments were made when the second interval was longer than the standard interval (1 sec.) than when it was shorter. This should be expected, as Woodrow pointed out,[5] because all of the intervals used here were above the indifference point and therefore gave the "absolute impression of long," even though they were relatively short. This would tend to minimize the difference when the second interval was shorter and exaggerate it when the second interval was longer. During the experiment, the second interval was shorter half the time and longer half the time, so that any error due to position was constant.

The problem of disposing of "equal" judgments has long been a topic of dispute. There are three possibilities: (1) to disregard them entirely in the final results; (2) to divide them equally between right and wrong judgments; and (3) to exclude them, limiting the *O*s to two categories of report.

The first solution (namely, to discard them from the results) is certainly unscientific. It is a selection of the data. By such a procedure, we should eliminate from ⅓ to ½ of the *O*'s judgments, as shown by a short experiment in which equal judgments were admitted. These *O*s had made several thousand judgments previous to this experiment and were consequently well trained. Furthermore, they were persons who were interested; they volunteered for these extra sittings.

The second method of disposing of the "equal" judgments; namely, to divide them equally between the correct and incorrect judgments, has some vogue. However, it can be argued that it is better to allow *O* to divide them between the two categories according to slight or even subliminal clues than for *E* to divide them equally.

An experiment in which equal judgments were allowed was repeated using 3 *O*s. Of the 1695 judgments, 42% were "equal." Thresholds were computed both by disregarding these "equal" judgments and by dividing them equally between the right

[5] H. Woodrow, Behavior with respect to short temporal stimulus-forms, *J. Exper. Psychol.*, 11, 1928, 167-198.

and wrong responses. Table I shows considerable variation depending upon the method. It would be difficult to judge which figure is the nearest to the true threshold.

TABLE I

SHOWING THE DIFFERENT THRESHOLD OBTAINED BY EACH OF THE THREE METHODS OF DEALING WITH "EQUAL" JUDGMENTS

Equal judgments	Audition	Vision	Touch
(1) Not counted in final results	.074	.124	.087
(2) Divided between right and wrong	.078	.126	.088
(3) Not allowed	.059	.118	.094

As Woodworth says, there are practically no cases in which *O* cannot find some difference in two stimuli if he is required to search for it.[6] Therefore there can be little justification for the admission of the equality judgment. The number of equal judgments made always depends upon the standard of clearness or confidence *O* sets up as boundary between equal and different. An *O* who is cautious and insists on a clear difference before he makes the judgment longer or shorter will show a broad middle zone, where he is unable to decide the direction of the difference and so classifies the stimuli as equal. The same *O* by following every little clue and slight impression will greatly reduce that broad zone of equal judgments. Consequently the threshold will be reduced.

In view of the arguments and experimental evidence presented above we decided to use only the two-category plan in the remainder of our research. Some provision for doubtful cases was provided in that *O* was not compelled to respond every time, but was allowed a second presentation of an interval-difference if he was entirely unable to decide at the first presentation. However, this was not encouraged and the majority of *O*s used it only occasionally.

It will be recalled that the Method of Right and Wrong Cases was only one-third of this research, and since we had to determine psychometric constants for 3 sense modalities, some cut in the time required of volunteer *O*s had to be made. The only alternative possible would have been the use of one or two *O*s who might have been persuaded to give the necessary 30 hours for the experiment. Since the principal object of all this research was a comparison of the 3 different modalities, a larger number of *O*s seemed absolutely indispensable. In view of this fact, we decided to reduce by more than half the time required of our *O*s in this part of the experiment, by working with only 2 intervals rather than 5 or 7 as is customary in psychophysical techniques. During the training period (which was considerable) we estimated the threshold value. Then by using time values which would include the estimated threshold, we proceeded to take judgments on those time intervals. In a few cases, we found that the intervals did not include the threshold and therefore we had to add a third interval. The thresholds were computed by the formula: $D.L. = [D_a (B - 50) - D_b (50 - A)] / (B - A)$ where B represents the percentage of correct judgments at the interval D_b and A the percentage of correct judgments at the interval D_a. Of course this assumes that the curve rises at a uniform rate from D_b to D_a, which is not correct; but as the intervals D_b and D_a were quite

[6] R. S. Woodworth, The psychological researches of James McKeen Cattell, *Arch. Psychol.*, 4, 1914, (no. 30), 60-74.

close (20 σ apart) the error incurred by not considering the shape of the curve is slight. These data do not make it possible to compute *h*, the constant of precision, which would be an interesting bit of knowledge, but it can hardly be considered essential for our purposes. Some measure of the relative spread of the judgment is afforded by the slope (M) of the curve when the responses are plotted against the stimulus-values which is given by the formula: $M = dy/dx$. Table II shows the threshold values and the slope of the curve for each *O* in each modality.

TABLE II

SHOWING THRESHOLDS AND THE SLOPES OF CURVES FOR DATA OBTAINED FROM THE METHOD OF RIGHT AND WRONG CASES

O	Auditory		Visual		Tactual	
	D.L.	M	D.L.	M	D.L.	M
1	.071	6.5	.215	4.5	.102	6
2	.053	5	.093	3	.073	3
3	.113	5.5	.130	5	.260	5
4	.091	6	.138	4.1	.177	2.7
5	.047	8	.091	8	.069	4
6	079	5.5	.167	6.5	.148	7
7	.033	5.5	.114	3	.104	5
8	.092	4	.159	6	.134	6
9	.096	8	.134	8.5	.095	9
10	.046	10.5	.095	2	.080	3
11	.074	5	.201	7	.163	4.5
12	.084	6	.118	3.5	.100	8.5
13	.037	5	.102	4	.069	0.5
14	.061	5	.106	5	.056	4.0
15	.064	2	.164	4.5	.115	6.2
16	.071	2	.130	5	.127	2
17	.093	3.5	.195	5	.094	3.5
18	.053	7.5	.108	9	.053	7
Av.	.070	5.5	.137	5.2	.100	4.8

Just Noticeable Differences. Starting with the second interval unmistakably longer than the first, it was gradually reduced until *O* reported that the intervals had become equal. The midpoint between the smallest interval perceived as longer and the longest interval perceived as equal was taken as the upper threshold (R_{ud}) for the descending series. The second interval continued to grow shorter by imperceptible decrements until *O* reported that it was shorter. The midpoint between the smallest interval perceived as equal and the largest perceived as shorter was the lower threshold (R_{ld}) for the descending series. The interval was made still shorter so that the difference was clearly perceptible, and gradually increased until *O* reported equal. A point halfway between the largest interval perceived as shorter and the smallest interval perceived as equal gave us the lower threshold (R_{la}) for the ascending series, which when averaged with the R_{ld} (lower threshold for descending series) gave a value which we considered the lower threshold (R_l). The interval was increased farther until *O* reported that the second interval was longer, and an upper threshold computed for the ascending series which, averaged with the value obtained in the descending series, gave the upper threshold (R_u). The two limens were computed 5

times (in several cases 10 and even 15 times) for each *O*, and the values given here are the average thresholds.

The value R_u above which an interval is perceived as longer, and the value R_l below which an interval is perceived as shorter, than the standard 1-sec. interval determine the boundary of the period of uncertainty.

As has already been point out, all of the intervals used in this study are long "absolutely;" *i.e.* they are all above the indifference point (approximately 0.6 sec.) so that they give the "absolute impression of long." Therefore, when the variable interval becomes less than the standard, it is overestimated, because it is long "absolutely," although short compared with the standard. As a result, R_l is farther below standard than R_u is above it. Therefore, probably the R_u would be a truer threshold than the average of R_u and R_l. The opposite point of view is just about as tenable. We will present all the data and let the reader take his choice.

The estimation value of the stimulus (R_e) is another psychophysical constant which will be used later in comparison with the average R obtained by the method of reproduction. The value, designated by R_e, is merely the average of all the 8 values obtained in computing the thresholds. Since thresholds were computed 5 times, each value (R_e) represents the average of 40 values.

Table III shows the upper and lower threshold values, average threshold value, and probable errors for each *O* as computed by the Method of Just Noticeable Differences.

Method of Reproduction. Our third approach to a comparative study of the 3 different modalities was by the Method of Average Errors, or more appropriately called in this research, the Method of Reproduction. *O* was given 2 stimuli marking off the standard 1-sec. interval, and asked to produce the third stimulus so as to make the first and second intervals equal. The third stimulus which *O* produced was exactly the same qualitatively and quantitatively as the first two produced by the electric impulse distributor. For each response *E* recorded the error and its direction. In this experiment, the only motor response required by *O* was depressing a key about 1/16 in. After our *O*s had practiced placing the third stimulus in the proper place, they were instructed to put their attention on the intervals which they produced rather than upon the actual pressing of the key. Thus we hoped to reduce the interference which the motor adjustment made with the placement of the final stimulus.

However, this method of reproduction is hardly comparable with the previous method, since here we are measuring not *O*'s discrimination but the precision with which he can reproduce an interval. There are certain interesting constants which can be calculated from these data. First of all, there is the length of the reproduced interval which is generally designated by R, and is given by the formula: $R = 1.00 - \Sigma E/N$; in which E represents the actual error made on each trial, and N the number of trials. From this value, we can compute C, the constant error, by the formula: $C = R - 1$.

A more significant factor is the average error, A.E., which is merely $A.E. = \Sigma E/N$ without regard for signs. Such a constant is not worth much without some measure of reliability, so we have included the probable error of the average for each *O*, which is $P.E._{av} = P.E./(N)^{\frac{1}{2}}$.

The average errors contain, in addition to a constant error (C) a variable error (V.E.) which may be computed by $V.E. = (E - C)/N$. The 5 constants just described have been computed for each sense modality and are given in Table IV.

TABLE III

SHOWING PSYCHOMETRIC CONSTANTS OBTAINED FOR EACH MODALITY BY THE METHOD OF JUST NOTICEABLE DIFFERENCE

O	Audition				Vision				Touch			
	R_u	R_l	D.L.	P.E.	R_u	R_l	D.L.	P.E.	R_u	R_l	D.L.	P.E.
1	.040	.068	.054	.002	.052	.056	.054	.001	.052	.068	.060	.002
2			.035				.034				.033	
3	.100	.036	.068	.005	.110	.116	.113	.003	.100	.078	.089	.004
4	.092	.098	.095	.005	.116	.120	.113	.003	.096	.128	.114	.002
5	.032	.014	.023	.001	.180	.036	.108	.008	.100	.020	.060	.005
6	.034	.052	.043	.001	.124	.084	.104	.004	.044	.052	.048	.002
7	.100	.060	.080	.003	.252	.092	.172	.008	.260	.100	.180	.008
8	.100	.055	.077	.003	.090	.100	.095	.002	.085	.120	.102	.002
9	.080	.023	.076	.003	.134	.102	.118	.003	.106	.123	.114	.002
10	.040	.034	.037	.001	.066	.056	.061	.002	.056	.043	.049	.002
11	.126	.060	.093	.003	.166	.153	.159	.003	.126	.113	.119	.002
12	.036	.068	.052	.003	.084	.120	.102	.003	.084	.068	.076	.002
13	.028	.034	.031	.002	.067	.082	.074	.002	.059	.060	.054	.002
14	.084	.028	.056	.003	.120	.072	.096	.005	.100	.036	.068	.003
15	.068	.060	.064	.004	.060	.076	.068	.003	.048	.076	.062	.002
16	.124	.044	.084	.004	.188	.084	.136	.004	.148	.044	.096	.005
17	.076	.068	.072	.003	.148	.156	.152	.003	.132	.108	.120	.004
18	.036	.052	.044	.002	.100	.076	.088	.002	.060	.060	.060	.002
Av.	.070	.053	.061	.003	.121	.093	.103	.004	.097	.076	.084	.003

TABLE IV

SHOWING PSYCHOMETRIC CONSTANTS OBTAINED FOR EACH MODALITY BY THE METHOD OF REPRODUCTION

O	Audition				Vision				Touch			
	A.E.	P.E.	V.E.	C.E.	A.E.	P.E.	V.E.	C.E.	A.E.	P.E.	V.E.	C.E.
1	.160	.001	−.100	.090	.161	.0024	.125	−.036	.114	.004	.86	.028
2	.064				.078				.078			
3	.071	.003	−.056	.005	.096	.0049	.045	.051	.096	.005	.060	.036
4	.070	.004	.060	.010	.115	.0062	.064	.051	.117	.004	.087	−.030
5	.044	.003	.035	.009	.057	.004	.022	.035	.094	.005	.021	.073
6	.068	.003	−.019	.049	.081	.004	.043	.038	.059	.004	.045	.014
7	.063	.002	.006	.057	.074	.005	.047	.027	.068	.004	.083	.003
8	.180	.009	−.098	.082	.256	.005	.000	−.128	.120	.005	.000	−.120
9	.083	.004	.062	.021	.111	.005	.052	.059	.089	.005	.040	.049
10	.091	.005	.074	.017	.104	.005	.100	.004	.093	.004	.047	.046
11	.102	.005	−.101	.001	.152	.004	.0000	−.152	.181	.000	.000	.181
12	.076	.003	−.035	.041	.117	.004	.069	−.048	.142	.004	.030	−.112
13	.045	.003	.038	.007	.117	.004	.017	.100	.049	.004	.023	.026
14	.051	.004	−.049	.002	.083	.005	.025	−.058	.079	.002	.004	−.075
15	.115	.005	−.032	.082	.118	.005	.031	−.087	.090	.003	.053	−.037
16	.129	.003	−.103	.026	.108	.004	.074	.034	.114	.004	.020	−.094
17	.125	.004	−.124	.001	.119	.005	.031	−.088	.140	.005	.003	−.137
18	.042	.002	.006	.036	.126	.003	.000	−.126	.074	.004	.010	−.064
Av.	.070	.004	−.026	.027	.107	.004	.044	−.023	.100	.004	.036	−.034

Comparison of the Psychological Methods Employed

The problem of the validity of the various psychophysical methods is not new in the history of experimental psychology. Kellogg,[7] in 1929, made an elaborate investigation using all three methods. He found that the Method of Reproduction was the finest measure of differential sensitivity since it gave the smallest variable error. The Method of Constant Stimuli gave the most reliable results, *i.e.* the smallest variation from sitting to sitting.

It is difficult to determine the relative validity of the three techniques used in this research because of the lack of adequate criteria. However, we can compare the results obtained from the different psycho-physical methods, and in that way infer something about their relative validity.

In the first place, all three methods give the same results in that they showed hearing to have the lowest differential threshold, and vision the highest. Table V shows the average results obtained from each method.

Another method of comparing the technique is by means of correlation coefficients which, because of the small number of cases, were computed by the Rank-Difference Method.

TABLE V

Showing Differential Threshold Obtained from Each Method for Each Modality

Method	Audition	Vision	Touch
Right and Wrong Cases	.070	.137	.100
Just Noticeable Differences	.061	.103	.084
Reproduction	.080	.107	.100
Av.	.070	.115	.095

TABLE VI

Coefficients of Correlations Showing the Relationship of the Different Technique

	R. & W. with J. N. D.	R. & W. with Reproduction	J. N. D. with Reproduction
Audition	$.51 \pm .12$	$.46 \pm .11$	$.55 \pm .12$
Vision	$.16 \pm .15$	$.62 \pm .10$	$-.08 \pm .16$
Touch	$.38 \pm .13$	$.53 \pm .11$	$.73 \pm .09$

It will be observed that in audition, the three correlations are all sufficiently larger than their probable errors. In vision, however, a very different situation exists. The Methods of Reproduction and Just Noticeable Difference are correlated to the extent of —0.08, so obviously, both cannot be a reliable measure of differential sensitivity. We find that the Method of Right and Wrong Cases is correlated to a greater extent with the Method of Reproduction (0.62) but not with the Method of Just Noticeable Dif-

[7] W. N. Kellogg, An experimental comparison of psychophysical methods, *Arch. Psychol.*, 17, 1929, (no. 106), 1-86.

ference (0.16). This seems to cast doubt on the validity of the latter method as a measure of differential sensitivity in the field of vision.

The Rank-Difference Method of correlation conceals much of the relationship because of the proximity of the threshold values, but, with only 18 cases, it was the only method possible. A truer picture of the real relationship can be had from an inspection of Table VII.

TABLE VII

SHOWING DIFFERENTIAL THRESHOLDS FOR EACH MODALITY AS COMPUTED BY EACH OF THE THREE PSYCHOPHYSICAL METHODS

O	R. & W. Cases			J. N. D.			Reproduction		
	A.	V.	T.	A.	V.	T.	A.	V.	T.
1	.071	.215	.102	.054	.054	.065	.117	.161	.114
2	.053	.093	.073	.036	.034	.035	.064	.078	.078
3	.113	.130	.099	.068	.113	.089	.071	.096	.096
4	.091	.138	.117	.095	.118	.112	.098	.107	.117
5	.047	.091	.069	.023	.108	.060	.044	.057	.094
6	.097	.167	.148	.043	.104	.048	.068	.081	.059
7	.033	.114	.104	.080	.174	.180	.063	.074	.086
8	.092	.159	.134	.077	.095	.102	.079	.256	.240
9	.096	.134	.096	.076	.118	.113	.083	.111	.089
10	.046	.095	.080	.037	.061	.049	.091	.104	.093
11	.074	.201	.163	.093	.159	.120	.102	.152	.181
12	.084	.118	.100	.052	.102	.076	.054	.096	.117
13	.037	.102	.069	.031	.074	.055	.045	.117	.049
14	.061	.106	.066	.056	.096	.068	.051	.083	.079
15	.064	.164	.115	.064	.069	.058	.113	.118	.090
16	.071	.136	.127	.084	.136	.096	.129	.108	.128
17	.093	.195	.094	.072	.152	.120	.125	.119	.140
18	.053	.108	.053	.044	.088	.060	.042	.126	.074
Av.	.070	.137	.100	.061	.103	.084	.080	.107	.100
A.D.	.018	.034	.029	.019	.033	.029	.024	.027	.030

A final method of comparing the reliability of the techniques is a study of the changes in the results obtained for 6 *O*s from two sittings a month or more apart, and is shown in Table VIII.

TABLE VIII

SHOWING THE AVERAGE CHANGES IN DIFFERENTIAL SENSITIVITY OVER A PERIOD OF A MONTH

Modality	R. & W.	J.N.D.	Reproduction
Audition	.016	.002	.059
Vision	.039	.012	.052
Touch	.037	.018	.064
Av.	.031	.011	.058

COMPARISON OF DIFFERENTIAL SENSITIVITY IN THE THREE MODALITIES

As has already been observed (Table V), all three techniques show audition to have the keenest differential sensitivity and vision the poorest, with touch lying midway between audition and vision.

Another comparison is afforded by a comparison of the variability of the *O*s for the different sense modalities. Just as the measures of central tendency show the order of differential sensitivity to be audition, touch, and vision, so the following table shows the amount of variability as measured by the average deviation from the mean to be in the same order.

TABLE IX

SHOWING THE AVERAGE DEVIATION FROM THE MEAN FOR EACH MODALITY AS MEASURED BY THE DIFFERENT METHODS

Method	Audition	Vision	Touch
Right and Wrong Cases	.018	.034	.024
Just Noticeable Differences	.019	.033	.027
Reproduction	.024	.029	.030
Av.	.020	.032	.027

It is interesting that keen differential sensitivity is accompanied by less variability, so that we can say that the greatest sensitivity and the least variability are found in audition, while the very opposite is true of vision. Touch lies midway between audition and vision on both scores—sensitivity and variability. The correlation coefficients in Table VI show that the degree of correlation among the different methods is quite constant for audition and quite variable for vision. As usual, touch stands midway between audition and vision. This would lead us to place most confidence in the measurement of auditory differential sensitivity and least in the visual field.

If we now turn to the individual *O*s, we can make a comparison of the variability in the different sense modalities. Here too, we find that the responses to the auditory stimuli are most reliable (as measured by the probable error of the average) and those to the visual stimuli least. The reliability of responses in the tactual situation is between that for vision and audition. Tables III and IV show the probable error of the average for each *O* for the Methods of Just Noticeable Differences and of Reproduction.

TABLE X

SHOWING THE P.E.$_{av.}$ OF THE AVERAGE FOR THE DIFFERENTIAL THRESHOLDS AS OBTAINED FOR EACH MODALITY BY THE METHODS OF REPRODUCTION AND JUST NOTICEABLE DIFFERENCES

(The P.E.$_{av.}$ for each will be found in Tables III and IV)

Method	Audition	Vision	Touch
Reproduction	.0041	.0047	.0042
Just Noticeable Differences	.0028	.0035	.0030

To summarize, we can say that on the average the differential threshold for vision is 65% higher and for touch 34% higher than the threshold for audition.

TABLE XI

SHOWING THE RELATIVE VALUE OF DIFFERENTIAL THRESHOLDS FOR EACH MODALITY AS OBTAINED BY THE DIFFERENT TECHNIQUES

Method	Audition	Vision	Touch
Right and Wrong Cases	1	1.98	1.44
Just Noticeable Differences	1	1.65	1.33
Reproduction	1	1.34	1.25
Av.	1	1.65	1.34

In comparing the variability and the reliability of the three modalities, we found the same order to hold. The following table shows the relative variability of the different sense modalities.

TABLE XII

SHOWING THE RELATIVE AVERAGE DEVIATIONS FOR EACH MODALITY

Method	Audition	Vision	Touch
Right and Wrong Cases	1	1.88	1.33
Just Noticeable Differences	1	1.72	1.45
Reproduction	1	1.11	1.25
Av.	1	1.57	1.34

In brief, audition gives the keenest differential sensitivity, is least variable, and most reliable. Vision gives exactly opposite results while touch lies between audition and vision on all three scores.

PROBLEM-SOLVING IN DIFFERENT FIELDS OF ENDEAVOR

By MARION LEROY BILLINGS, Western Kentucky State Teachers College

The term reasoning, like many other terms in psychology, has been used with a variety of meanings. It will be the purpose of the first part of this paper to discover the essentials of reasoning by reviewing the literature on the subject and then to conclude with a definition. The second part will give the data and conclusions of experiments dealing with "problem solving" in eight different academic fields.

The belief in mental faculties as separate entities promoted by the phrenologists and their followers is gradually giving way to the belief in general mental functions. That is, memory, for example, is a general function of the whole brain rather than any limited part of it. This belief is supported by the experiments of such men as Franz[1] and Lashley.[2] Lashley maintains, as a result of his experiments on rats, that the brain operates as a whole, and a habit which has been thoroughly established may be taken over by other parts of the brain, and perhaps, by the brain stem. He also concludes from his experiments that with any general or complex type of behavior the amount lost in the behavior by the removal of parts of the brain is in direct proportion to the amount of the brain removed without reference to the location. Such experiments tend to refute the idea of highly localized functional areas for the higher and more complex functions. Thus, we may conclude from a study of such experimental evidence that the various functions of the brain are very general functions involving probably the whole cortical portion if not the brain stem.

This conception of the brain and its functions does not imply, however, that the different functions such as memory, imagination, and reasoning may not vary greatly in their relative efficiency in the same individual.

* Accepted for publication August 12, 1932. This experiment has been carried through three different times: once at the University of Michigan in 1929, the results of which were submitted as a dissertation in partial fulfillment of the requirements for the degree of Doctor of Philosophy, and the other two times at the Western Kentucky State Teachers College in 1930 and 1931.

The author wishes to acknowledge his indebtedness to Professors W. B. Pillsbury and Carl Spearman and others who aided in the experiments and in the preparation of the manuscript.

[1] S. I. Franz and K. S. Lashley, The retention of habits by the rat after destruction of the frontal portion of the cerebrum, *Psychobiol.*, 1, 1917, 3-18.

[2] K. S. Lashley, *Brain Mechanism and Intelligence*, 1929, summary, 175-6.

That the brain functions as a whole does not oppose the idea that a given brain may vary markedly in its capacities to receive, to retain, or to relate. A certain brain may receive without retaining, or it may retain and recall without being able to relate; that is, to use in solving problems; but, if it retains well in one academic field, as this is a general function, it ought to retain well in any other, other things being equal. In other words, retention does not differentiate in favor of any field of material. Likewise, the brain that is apt in relating in one field may be relatively as efficient in all other fields, other things being equal. Therefore, the writer launched these experiments to test the thesis, "That a person will be as good a reasoner in one academic field as another providing he has an equal background."

Essentials of Reasoning

In order to secure a definition of reasoning consistent with the various authorities and to discover the conditions of reasoning, the steps in reasoning, and the essentials or nature of the reasoning process, the literature on reasoning has been reviewed.[3] As a result of this review we may make the following summary.

(1) The essential conditions for reasoning are: first, a problem; secondly, sufficient experiences with which to solve the problem; and thirdly, the individual must possess sufficient ability to rearrange those experiences in new relations to meet the new situation.

(2) The formal steps in reasoning as found in the literature are: a problem; an understanding of the problem, or judgment; the solution of the problem, or inference; a conclusion; a belief in the conclusion; verification; proof; and application. From the psychological point of view, the last three are not parts of the real reasoning process; but are follow-up processes for the purpose of testing or checking up on the conclusion, which really ends the reasoning process. Verification is but supplementary proof; application is but practical proof; and induction and deduction are the logician's method of proof.

(3) Thus, we may conclude that the essentials of reasoning may be reduced to: a problem, an understanding of the problem, and the solution of the problem, which presupposes essential experiences and ability.

(4) Finally, we may define reasoning as the solving of a practical or theoretical problem or difficulty by the use of or through the relating of past experiences, the end result of which is believed, no matter how fallacious or inconsistent it may seem to others. One accepts it as true because it is consistent with what one knows. (Frequently, our *Ss* who failed completely in solving the problems were very positive of their results.)

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[3] A fuller treatment of this subject may be obtained from the University of Michigan in the original thesis.

Experiments in Problem-Solving

Object. The following experiments were planned to test the assumption that problem-solving, which involves the mental relating of experiences, is a general function and constant within a given individual, other things being equal.

To do this 146 *Ss* in groups of 50, 46, and 50 were ranked according to their ability to solve problems in eight academic fields: geometry, arithmetic, physics, mechanics, economics, sociology, geography, and history. In order to test ability and not past experiences, all of the materials necessary for solving the problems in each of the fields were thoroughly taught. Then the ability to solve problems with these materials was tested according to the usual classroom procedure. The *Ss* were allowed to use only the materials taught for solving the problems. If other materials were used, the solutions were not counted.

Materials and Technique

Material. The following are fair samples of the various kinds of materials used:

Geometry. If a line from the vertex of an isosceles triangle is perpendicular to the base, prove that it bisects the base and the vertex angle.

Prove that the bisector of the exterior angle at the vertex of an isosceles triangle is parallel to the base.

At noon, a flag pole casts a shadow 56 ft. along the ground. How tall is the pole? The only instrument that you are allowed to use is a yard stick. When the yard stick is stood up along side and parallel to the pole it casts a shadow 16 in. long. The ground is level and at right angle to the base of the pole.

Arithmetic. A steer dresses away 35%. If it weighs 877.5 lb. after it is dressed what did it weigh alive?

What should be the selling price per pound for a blended coffee blended in the following way: 20% of 50¢ coffee, 33⅓% of 60¢ coffee, and 46⅔% of 30¢ coffee?

The freezing and boiling points on a Centigrade thermometer are 0° and 100° respectively. The same points on a Fahrenheit thermometer are 32° and 212° respectively. What will be the reading on the Centigrade thermometer when the reading on the Fahrenheit thermometer is 72°?

Physics. What effect will a double concave lens have on the direction of propagation of two parallel rays of light? Why is this so?

A locomotive passes an observer. The pitch of the whistle changes as it goes away. Does the pitch rise or lower? Why?

A steamboat has a water displacement of 1000 cubic feet; the boat itself weighs 5 tons; how many tons of cargo can the boat support?

Mechanics. In the transmission of an automobile are three gears arranged on the drive shaft from the engine from front to back: an 8 in. gear, a 6 in., and a 4 in.; and on the drive shaft leading to the differential in the same manner: a 4 in. gear, a 6 in., and an 8 in. (In the original the mechanism of the transmission and the differential were drawn.)

(a) Which two gears would you mesh to get the greatest power at the wheel?
(b) Which two gears would you mesh to get the greatest speed of the wheel?
(c) With the same gears meshed, which would give the greatest speed along the road, a 26 in. wheel or a 32 in. wheel?

(d) With the same load, which size wheel will take the most power to drive, the 26 in. or the 32 in.?

(e) Suppose that your car is standing on an incline headed down hill. You are going to start your engine by allowing your car to coast down the incline. Which gears will you mesh in order to have your engine start the easiest?

Economics. "Internal commerce does not increase the wealth of a nation since it only transfers goods from one person to another." Is this sound? Why?

"If the wheat crop of the world should fall off one half next year, a rise in price would be of a great social advantage." Is this sound? Why?

Why is a monetary system based on a bi-metallic standard unsatisfactory?

Sociology. Why is the rule of the Clergy in Civil Society objectionable?

Why is organized and synchronized work in education and industry apt to cause many human failures?

Why is competition (struggle) in society, on the whole, a beneficial process?

Geography. Why is the earth cold at the poles and hot at the equator? (This is not due to the poles being farther from the sun).

What would be the most evident effect if the axis of the earth was not inclined to its plane of revolution?

What is happening in our meteorological conditions when the winds shift in the following way: N.E. to N. to N.W.?

History. Why did the New England states become manufacturing states and the Southern states agricultural states?

How could Whitney's invention of the cotton-gin in 1792 be given as one of the causes of the Civil War?

What effect did the Emancipation Proclamation have on England's behavior toward the South? Why?[4]

Procedure. (1) Instructions. Printed instructions were placed in the hands of the *S*s before every test. To insure uniformity of understanding, *E* read them aloud to the *S*s and explained such details as were necessary. The instructions were as follows:

Please remember that your value as a subject in this experiment depends upon your faithfully carrying out the instructions given you. The purpose of this experiment is to obtain reliable data for whatever conclusions it may warrant. Your contribution to the results will depend upon your putting forth each time your best efforts. (1) You will be presented with some material which you will try your best to master. (2) You will be given some questions to answer to test how well you digested the material. (3) You will be given some problems to solve based upon this material. After the experiment has been finished, the aim and results will be given you.

Avoid worrying while working on the material. You will not be expected to get all of the problems; that is, not all of you. What is expected of you is that you do your very best. Consistent effort throughout the experiment is what will give the best results rather than solving every problem.

If any of the problems are so familiar to you that you do not have to solve them, that is, you do not have to think them through but you can put down the answer from memory, indicate this in writing in the margin beside the problems. It may be that you have had something similar before, but if enough time has elapsed so that you actually have to work the problems through again to solve them, then they are the same as new. Only in the cases that you know the answers without having to work the problems through will you indicate that you did not have to solve them.

[4] An attempt was made to avoid as far as possible an overlapping of subject matter, such as arithmetical computations in geometry, physics, economics, etc.

You will accept the material taught as true for the solving of the problems. We wish to test your ability to use the material as given.

You will use *only* the materials taught for the solution of these problems. The reason for this is that we want all of you to work with the *same* materials and we wish to see how well you can do with materials as taught.

Special instructions for the problems in practical mechanics.

When working the problems in practical mechanics do not motion out the solutions with the hands and do not use pencil and paper excepting as indicated. Make a complete solution of a problem in your head before you make a mark; and then do just what is asked of you and no more. Avoid erasing if possible. Be sure that you have your solution and then put it down. Further instructions will be given later.

(2) Individual information. The *Ss* were required to fill out blanks after every test period, giving the following information about themselves: name; sex; time consumed in solving the problem; year in college; previous preparation in the subject tested in semester hours in high school and in college.

(3) Routine of work. The material for each subject was thoroughly taught on Monday afternoon from 4–6 o'clock, or longer if needed. The *Ss* were held until they thoroughly understood everything that had been presented. Much of the material was simple and only a review for most of the *Ss*; but nothing was taken for granted. An outline of the material presented in each subject was given to each *S* so that the work might be reviewed before the test and vague points cleared up.

On Wednesday afternoon of the same week, from 4–6 o'clock, the tests were given. For the first 15 min., or more if needed, the *Ss* answered a true-false test on the subject matter to determine whether or not they had mastered the information presented. Most of the *Ss* were through with this in 5 min. If any points were unclear, they were explained and then the *Ss* were given the problems with these final instructions.

You will have but one hour to work on the problems, but do not worry about the amount you are getting done, just work as rapidly as is consistent with accurate work; you may work them in any order that you please; and you are to use for your solutions nothing but the material taught on Monday. Any fact which you have forgotten or of which you are not sure that is essential in the solution of a problem will be supplied you. You are to put down on your test paper all of your work; just answers do not count. Put down each step in your solution, for it is the solution in which we are interested and not merely the answer. Do not quit until the hour is up unless the problems have all been solved. Keep on trying.

(4) Evaluating the solutions. The steps necessary in the solution of each problem were decided upon by *E* in collaboration with two professors in each of the subjects tested. Two methods of evaluating the solutions given by the *Ss* were tried: an 'all-or-none' method (*i.e.* all right or all wrong), and a partial value method (*i.e.* giving part credit). Since it made little difference in the final outcome which method was used we evaluated the solutions by taking each step into account. If all the steps were correct, the solution was given full credit even though the final answer was wrong. Our interest was in the solution and not in the answer. Frequently answers would be far wrong because of errors in simple calculations while the principles of the solutions were entirely correct.

The relative difficulty of the problems and the values assigned them were determined as follows. If a problem was solved by all of the *Ss* it was given the lowest rank, 0. If 90% of the *Ss* solved it, it was ranked as 10; if 80% solved it, it was

ranked as 20; if 50%, as 50, and so on throughout the entire list of problems. After the problems had been ranked, arbitrary values were assigned them so as to bring the total number of points to 120. This was done, in lieu of standardized tests, in order to make them as objective as possible.[5]

(5) Attitude of the Ss. The general attitude of the *S*s was good. A few who did not take the work seriously were excused after the second meeting; the rest entered into the spirit of the experiment and worked hard.

(6) Subjects. Of those completing the tests, the first group was made up of 30 men and 20 women, the second group of 24 men and 22 women, and the last group of 18 men and 32 women—146 *S*s in all. They were selected almost entirely from the classes in psychology in the University of Michigan and the Western Kentucky State Teachers College. There were 12 seniors, 34 juniors, 58 sophomores, and 42 freshmen.

RESULTS

(a) Subject matter. The subject matter or information taught was well mastered. From 23 to 36 'true-false' questions were used to test the *S*'s knowledge of each of the subjects taught. An average of only 2.22 questions was missed on all of the tests.

(b) Problem-solving. The tables below give the correlations between every two of the eight academic subjects used in this study. It will be noted (1) that these correlations are moderately high and constant, and (2) that the inter-correlations between the science and the humanitarian groups are about as high and as constant as the intra-correlations within each of these groups.

Some of the correlations were lowered by the following factors over which we had no control.

(1) The correlations were most affected by the fact that many of the *S*s were taking college courses in the subjects employed in this study; for example, of the first group of *S*s, 13 were taking courses in economics, 10 in sociology, 10 in history, 4 in physics, 3 in geography, and none in geometry or mechanics. The other groups of *S*s gave similar results. In order to demonstrate the effect of simultaneous study upon the correlations, the *S*s taking economics and sociology were omitted, and a few of the correlations were recomputed. The correlation between geometry and economics rose from 0.55 to 0.69; between arithmetic and economics, from 0.60 to 0.67; between mechanics and economics, from 0.65 to 0.74; and between geometry and sociology, from 0.41 to 0.69. These values are given in parentheses in Table I.

(2) Another factor which affected geometry slightly was the inaccuracy in calculations; *e.g.* in some cases errors were made in calculating the degrees of the angles not given in the problems, which made the solutions of the problems impossible with

[5] We had hoped to use standardized tests in these experiments, but we were unable to find any that were suitable. Few tests are concerned with problem-solving, most are based on memory—even in mechanics and physics, the task usually set is to identify or label parts or wholes.

the material given. Had these angles been correctly calculated, the solutions would have been comparatively easy.

TABLE I

CORRELATIONS FOR FIRST GROUP OF *Ss*

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.69						
Physics	.66	.77					
Mechanics	.65	.67	.77				
Economics	.55 (.69)	.60 (.67)	.69	.65 (.74)			
Sociology	.41 (.69)	.59	.65	.65	.72		
Geography	.48	.58	.68	.60	.57	.62	
History	.58	.60	.63	.65	.69	.78	.56

TABLE II

CORRELATIONS FOR THE SECOND GROUP OF *Ss*

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.78						
Physics	.73	.79					
Mechanics	.47	.57	.66				
Economics	.69	.74	.77	.54			
Sociology	.67	.74	.78	.57	.90		
Geography	.61	.72	.84	.68	.77	.81	
History	.60	.70	.81	.61	.80	.89	.86

TABLE III

CORRELATIONS OF VALUES FOR ALL THREE GROUPS TOGETHER
(Raw correlations without corrections)

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.76						
Physics	.78	.75					
Mechanics	.61	.63	.77				
Economics	.70	.66	.78	.59			
Sociology	.64	.69	.78	.65	.86		
Geography	.59	.68	.77	.69	.71	.75	
History	.53	.65	.63	.56	.66	.75	.68

(3) A third factor which affected the results slightly was the fact that two of the *Ss* did not follow the instructions. For example, one *S* who was antagonistic to the social principles taught spent the most of the hour attacking those principles rather than trying to solve the problems. Thus, this *S* ranked high in everything but sociology. In an interview later, he had no difficulty in solving the problems.

In the opinion of the writer these correlations are very significant considering all of the factors contributing to lower them. Besides those mentioned above, the following could not be entirely eliminated in a laboratory experiment: *(4)* earlier established attitudes; *(5)* short periods for instruction, and *(6)* the specializing of some of the *Ss* in some of the academic subjects.

(c) Coefficient of reliability of the problem-solving tests. Spearman has recommended, when a test cannot be repeated that the odd problems be correlated against the evens and then applied to the formula: $r_x = 2rh/(1 + rh)$. This has been done, and the following table gives the raw correlations and the coefficients of reliability for each of the tests according to this formula.

(d) Correction of coefficients of correlations for attenuation. The coefficients of correlation for problem-solving given in Table III have been connected for attenuation according to Spearman's formula and are given in Table V.

TABLE IV

COEFFICIENTS OF RELIABILITY FOR THE PROBLEM-SOLVING TESTS

Subject	Raw correlation	Coefficient of reliability
Geometry	.85	.918
Arithmetic	.81	.895
Physics	.67	.802
Mechanics	.82	.901
Economics	.78	.876
Sociology	.74	.850
Geography	.75	.856
History	.52	.684

(e) Individual rankings. In order that the details of the experiment may be presented more clearly, the data have been analyzed further by giving the ranking of 10 Ss, selected haphazardly from the first group, in each of the eight academic subjects. Number 1 represents the highest rank and 50 the lowest.

TABLE V

CORRELATIONS OF TABLE III, CORRECTED FOR ATTENUATION

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.83						
Physics	.90	.88					
Mechanics	.67	.70	.90				
Economics	.77	.74	.92	.66			
Sociology	.72	.79	.95	.74	1.00		
Geography	.65	.77	.92	.78	.82	.83	
History	.79	.83	.86	.72	.85	.98	.88

TABLE VI

RANKING OF 10 SS OF THE FIRST GROUP

S	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.	Hist.
2	49*	47*	40	44	40	44	36	35
4	43	48	47	48*	47	36†	41	42
6	46	50	50	49	40*	37	49	34
12	42	43*	49	43	46	42	29	46
21	50	43*	41	48*	44	40	44	50
31	49*	47*	46	50	50	50	50	49
49	1	1	4	6	3	31‡	3	2
25	7	2	3	2	25§	1	1	3
26	3	10	19	10	11	13	5	11
28	12	13	1	5	1	4	2	4

* The reason for 2 Ss getting the same rank is that they obtained the same score in these tests.

† This S was taking sociology at the time of this experiment.

‡ This S is the one who attacked the principles of sociology instead of solving the problems.

§ This S received a grade of 70 in economics, which was a fair score; but it gave him a low rank because of the large number that were taking economics at the time and were crowded toward the top.

In only a very few cases did an *S* who ranked in the first quartile of one subject get into the fourth quartile of another and vice versa.

(f) Information and problem-solving. The following correlations were obtained between rank in information and in problem-solving: in geometry, 0.37; arithmetic, 0.35; physics, 0.59; mechanics, 0.49; economics, 0.47; sociology, 0.45; geography, 0.51; and history, 0.38. These correlations are lower on the whole than those obtained between the various problems.

An analysis of the data shows that the *S*s who were able to solve the problems had the required information, but not all the *S*s who had the information were able to solve the problems. The ability to solve the problems does not vary directly with the amount of information. As one *S* (a woman) reported, "I can learn all that you teach in a few minutes but I do not seem to be able to apply it in solving the problems"; and her results bore out the truth of this statement. She ranked among the highest in information, but only among the average in problem-solving. Furthermore, some of the *S*s, who made a poor showing in the information tests, did as well and in some cases better in problem-solving than other *S*s who stood high in information. Some *S*s cannot use what they know, while others use the knowledge they have to good advantage. One *S*, for example, of the first group was among the 6 lowest in information and the 12 highest in problem-solving.

(g) Effect of sex. Table VII shows that on the average the men ranked but a little higher in information and general intelligence, as measured by the Army Alpha Test, than the women but, in problem-solving, their average score was 48.7% above that of the women.

TABLE VII

A Comparison of the Scores Made by the Men and Women

Sex	Av. score information	Av. score prob.-solving	Av. Army Alpha	Av. I.Q.
Women	84.9	39.6	127.68	108
Men	90.2	58.9	141.76	114
Diff.	5.3	19.3	14.8	6
% diff.	6.2	48.7	11.0	5.5

(h) Correlations between problem-solving and the Army Alpha Test. The following coefficients show that there is a higher correlation between the problem-solving in these eight fields and the Army Alpha test than between the information tests in the eight fields and the Army Alpha.

(a) Coefficients of correlation between problem-solving in the eight fields and the Army Alpha Test are: geometry, 0.51; arithmetic, 0.58; physics, 0.55; mechanics, 0.59; economics, 0.42; sociology, 0.53; geography, 0.50; and history, 0.54.

(b) Coefficients of correlation between the information tests in the eight fields and the Army Alpha Test are: geometry, 0.31; arithmetic, 0.34; physics, 0.40; mechanics, 0.53; economics, 0.41; sociology, 0.19; geography, 0.39; and history, 0.31. This is probably due to the fact that 120 of the 212 points of the Army Test are scores for reasoning or seeing relations. The rest of the points are for information.

We find higher correlations between these 120 points and problem-solving of the eight academic subjects than with the information tests. Also, we find them higher than with the Army Test as a whole. (1) Coefficients of correlation between the 120 points of the Army Test and the problem-solving of the eight academic subjects are: geometry, 0.61; arithmetic, 0.68; physics, 0.69; mechanics, 0.64; economics, 0.59; sociology, 0.58; geography, 0.50; and history, 0.57. (2) The coefficients of correlation between the 120 points of the Army Test and the information test of these eight subjects are: geometry, 0.43; arithmetic, 0.32; physics, 0.43; mechanics, 0.52; economics, 0.34; sociology, 0.10; geography, 0.32; and history, 0.33.

(i) Correlations with previous preparation. In order to meet the criticism that the solutions of these problems might have been unduly influenced by previous training, correlations have been worked out between the solving of the problems and the previous preparation of the *S*s in semester hours in each of the subjects, except mechanics. Mechanics was excepted because of shop work, factory work, and home work which could not be reduced to semester hours. The average coefficient of correlation for the remaining subjects is 0.26 ranging from —0.17 to 0.53 as follows: geometry, 0.43; arithmetic, 0.50; physics, 0.53; economics, 0.30; sociology, —0.17; geography, 0.17; and history, 0.09.

The preparation in mathematics of various individuals varied from 12 to 36 semester hours, in physics from 0 to 15, in economics from 0 to 9, in sociology from 0 to 18, in geography and related subjects such as geology and physiography from 0 to 12, and in history from 6 to 36.

To show how little previous training influenced the results of these experiments, under the conditions given, the following 5 cases are cited. One *S* who had had no previous preparation in sociology received the highest score, and two others who had had no sociology tied for second and third places in problem-solving. Also, 2 *S*s who had had no previous preparation in geography made the highest scores in this subject. An exception to this general rule must be made in the cases of mathematics and physics. There is a higher correlation between these subjects and previous training, which may be due to the fact that students who are good in problem-solving elect these courses, since they are usually taught by the problem method and hence would be attractive to them.

(j) Correlations with honor points. There is a relatively low correlation between the average number of honor points earned in the University of Michigan and the scores made in solving the problems in these eight fields. The following are the coefficients of correlation: geometry, 0.34; arithmetic, 0.38; physics, 0.42; mechanics, 0.32; economics, 0.20; sociology, 0.42; geography, 0.42; and history, 0.32.

These results also show that those who received high honor points generally solved the problems, but the contrary is not true, not all of those able to solve the prob-

lems received honor points. This may be due to a number of factors of which the following three are but suggestions. (1) It may be due to the fact that many of the undergraduate courses are fact gathering courses; (2) it may be due to personal prejudices; or (3) it may be due to the fact that some of these students were not applying themselves to their university work.

The coefficient of correlation between the average scores made on the information tests in these eight subjects and the average honor points earned in the University of Michigan is 0.60. The coefficient of correlation between the average number of honor points and the Army Alpha scores is 0.50.

(k) Criterion of the tetrad equation. The correlations between the marks of school subjects, etc., tended toward an arrangement, which can be expressed in Spearman's tetrad equation.[6] He says:

> Whenever the tetrad equation holds throughout any table of correlations, and only when it does so, then every individual measurement of every ability can be divided into two independent parts which possess the following momentous properties. The one has been called the "general factor" and denoted by the letter *g*; it is so named because, although varying freely from individual to individual, it remains the same for any one individual in respect to all the correlated abilities. The second part has been called the "specific factor" and denoted by the letter *s*. It not only varies from individual to individual, but even for any individual from each ability to another.

Since these correlations meet the criterion of Spearman's "tetrad equation" it may be that problem-solving represents *g*, which Spearman says may be eventually appropriately called "intelligence."

Summary of Conclusions

This experiment has been repeated three times with consistent results during the last three years with three different groups of *S*s. Though we have no wish to be dogmatic, the following conclusions and generalizations seem nevertheless to be warranted.

(1) Given a common background, there is a rather high correlation between the ability to solve problems in one subject and the ability to solve problems in another. The average of the 29 correlations is 0.67.

(2) The correlations between some of the problem-solving tests were lowered by various factors which could not be eliminated in these experiments; the main one of which was that many of the *S*s were taking courses in similar subjects at the time of the experiments, especially economics, sociology, and history.

(3) These experiments show a lower correlation between information and the ability to solve problems in the same subject than between the ability to solve problems in different subjects. The average of these cor-

[6] C. E. Spearman, *The Abilities of Man*, 1927, 73 f.

relations is 0.45. However, those who solved the problems knew the material, but not all of those who knew the material could solve the problems.

(4) These correlations, as a whole, show very little relation between the ability to solve problems and previous preparation unless the material has been studied within a year. This is probably due not so much to extra information as to the similarity of the problems. When problems are similar and past solutions are fresh in mind, the right association or Gestalt is much more apt to come.

(5) The data herein given would indicate that the best fact gatherers receive the greatest number of honor points.

(6) Although the women ranked nearly on a par with the men in the Army Alpha and information tests, they were unable to maintain that equality in problem-solving. The men scored 48.7% higher than the women.

(7) These correlations meet the criterion of the tetrad equation.

The results of this study seem to substantiate the view that "problem-solving" is a general ability, "factor," or function. Thus, an individual, other things being equal, should function in this capacity relatively as efficiently in one field as another.

Although the functionalists and Gestaltists[7] differ in their general theories, these facts would accord with either of them if left to them to interpret. According to the first of these schools of psychology, problem-solving or the relating of past experiences to overcome a difficulty is a general function of the whole brain and depends upon the original mobility of the nervous system; while according to the second, especially to Köhler,[8] these inner workings are explained by internal dynamics of the nervous system, which depend upon the original dynamics for functional unity and organization. Both of these schools of psychology would explain problem-solving on the basis of some inherent disposition of the nervous system; one by trial and error or sudden unexplained association and the other by the physiology of the nervous system or inner dynamics. By either theory, however, the solution of a problem will be immediate and will depend upon past experiences either in the form of associations or functional units called patterns or configurations. As Wertheimer says,[9]

[7] Gestalt appears to be only a new interpretation or revised theory of association. Association is explained in functional wholes, configurations, or patterns as opposed to the serial a-b-c, type of association.

[8] W. Köhler, *Gestalt Psychology*, 1929, 103-149.

[9] M. Wertheimer, *Über Schlussprozesse im produktiven Denken*, 1920, 1-22; *Drei Abhandlungen zu Gestalttheorie*, 1925, 1-84.

problem-solving or creative thinking is the successful transposition of a member of one configuration to another. These different configurations are identified as belonging to a common configuration. Thus, these results and these theories would seem to make problem-solving a general function dependent upon the innate capacity or dynamics of the nervous system.

It is possible to explain the other mental processes in a similar manner; that is, they are general functions or manifestations and depend upon the original disposition of the physical organism. This theory makes it easy to explain why the same individual may differ so widely in his relative ability in the different functions. A "low grade" may have an excellent rote memory without being able to relate his experiences into a construct or to transfer them from one Gestalt to another. Each is a different function or manifestation of the brain. A piece of lead may receive impressions and retain them, but it is incapable of relating those impressions into anything different. Likewise, with these different functions of the nervous system, impressions may be received and retained without being related or organized. Of course, relating cannot be carried on without something to relate. Although memory may be independent of reasoning or problem-solving, problem-solving is more or less dependent upon a good memory.

Since memory is the more general function and much more easily measured than reasoning, it is easy to understand how our schools might unconsciously drift into the habit of exercising and testing this function. This combined with the bad habits of study acquired by many of our bright students may explain why the students good in memory receive the most honor points.

Finally, let us inquire as to what we mean by intelligence. What is it that differentiates the lower forms from the higher, the more intelligent humans from the less intelligent, the genius from the average, if it is not the ability to meet "unprecedented situations"? Such men as Socrates and Edison had fewer opportunities to acquire information than we; yet they rose head and shoulders above us. Why? Not because of their phenomenal memories but because of their ability to solve problems with what they knew. Is it not, therefore, this ability to solve problems which the makers of intelligence tests are trying to measure? If it is, then they shall have to revise materially their tests.

It is evident that these makers of tests are not agreed; for example, one group—Otis, Pressey, and those who helped to make the Army Alpha Test—excluded memory tests; another group—Binet, Terman, and Thorndike—include them. Yerkes, in one of his tests, has 6 components out of 20 given to testing the imagination; and the Army Alpha Test allows

ninety-two points (tests numbered 1, 4, and 8) of its 212 points for remembered facts. Who are right? Are all of these processes intelligence or are they but different manifestations of a nervous system? It is a well known fact that the lower forms of organic life have organic memories, but these cannot be classified as intelligent. What, then, is intelligence, especially in humans? Is it the ability to remember, of which "low grades" may have a liberal allowance, or is it the ability to use the memory facts in a practical or theoretical way to solve problems and to unravel the riddles of the universe? Stern says,[10] and the writer is inclined to agree with him, that intelligence is the mental power which produces conscious adaptations to new situations. Thus, it occurs to the writer that perhaps this series of tests or similar ones which will test the individual's ability to solve problems in the various fields of activity will be real tests of what we call intelligence and will some day replace the tests which deal with perception, memory, association, imagination, and muscular coördination.

In conclusion, we suggest that the ability to "solve problems," which is an important factor in Spearman's *g*, may be shown ultimately to be the distinguishing characteristic of, if not what we actually mean by, "intelligence" itself.

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[10] W. Stern, *The Psychological Methods of Testing Intelligence*, 1914, 3.

THE EFFECT OF A CONSTANT VISUAL ANGLE UPON THE BINOCULAR DISCRIMINATION OF DEPTH DIFFERENCES

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If we set up two pencils a little distance away and close first one eye and then the other, the perceived distance between the pencils will not be the same. This illustrates *parallactic difference* or *binocular parallax*. Other terms which have about the same meaning are *binocular disparity* and *stereoscopic acuity*. In the following pages the term *visual angle* has been reserved for the size of the retinal image.

History of the Problem. Although the physiology of binocular vision has had a long history, and the acuity of binocular depth-discrimination has been carefully studied, little attention has been paid to the effect of the changes in size of the visual angle of the objects.

Pulfrich[1] (1899) was the first to take this factor into account. He repeated Helmholtz's pin-experiment, using, instead of pins, stereoscopic pictures. In this way he kept the size of the retinal image of the objects constant. He determined that normal eyes could discriminate differences in depth when the parallactic difference between two objects was as low as 10″. Since Pulfrich gives little detail, one cannot be sure just what conditions did exist, and, therefore no definite conclusion can be drawn from his investigation.

Heine[2] (1900) used three rods as test-objects, their ends covered by a diaphragm. The middle rod alone was displaced, the outside rods remaining 5 meters from *S*. *Ss* with normal visual acuity were able to obtain a limen as low as 12″ in parallactic difference between the two objects; while those having sharp vision could still discriminate at 6″. Heine seems unaware of Pulfrich's problem.

Bourdon[3] (1900) made a careful study of stereoscopic acuity. He constructed a much more delicate apparatus which allowed the image of three pins to fall on the foveal region. The head was held firmly in position. When the stationary rods were two meters distant, he was able to appreciate a displacement of 1.5 mm. This is equal to a parallactic difference of 5″, which Bourdon found was the smallest value that he could obtain by the vernier method in monocular acuity. Bourdon does not mention the possibility of changes such as those in which we are interested.

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[1] C. Pulfrich, Über den von der Firma Carl Zeiss in Jena hergestellten stereoscopischen Entfernungsmesser, *Phys. Zsch.*, 1, 1899, 98.

[2] L. Heine, Sehschärfe und Tiefenwahrnehmung, *Arch. f. Ophthal.*, 51, 1900, 146. 51, 1900, 146.

[3] B. Bourdon, *La perception visuelle de l'espace*, 1902, 1-442.

The next important work was done (1919) by Howard.[4] He used only two rods, the ends covered and both movable, so that there was no constant point of orientation. The rods were placed at 6 meters and separated 6 cm. from each other. The images of both fell on the fovea. Using a questionable method, he found a 75% limen as low as 1.8″ for a few of his best *S*s. These results were obtained only with *S*s of high visual acuity and perfect balance of eye-muscles. He failed to take into account our factor of size of angle.

Andersen and Weymouth[5] (1923) redetermined the threshold of binocular depth-discrimination with normal vision. Threads were used as test-objects, the middle one movable. The threads were 2 meters from *S*, and were exposed for 1.5 sec. The 2-meter distance was alleged to reduce accommodation to a minimum. Since the movement of the middle thread is limited to 6 mm., the change in its apparent diameter was said to be too slight to be used as a cue. This latter claim is not substantiated. The limina lie at the points where the 50%-line crosses the curves of the distribution of the near- and the far-judgments; *i.e.* the 50%-line indicates the position where judgments of near and far equal half the total number of judgments at any position. This range from the point of 50% far- to 50% near-judgments was then divided by two in order to get the average single displacement. The authors who averaged their interpupillary distance before they derived any individual values. While there is no objection to deriving average values, this should be done after the final individual values have been calculated. They have assumed in their calculations, by dividing the above range by two, that the curves of the far- and near-judgments are identical in character and slope. As will be pointed out later in this article, this is not necessarily true; nor is there any constant relationship between the subjective and the instrumental zero. The curves are drawn from the summed results of all thread-lengths in each series. Their results on a thread-length of 2 cm. in series B have been used for comparison with our own. They were plotted and the values calculated in a manner identical with ours. The value obtained in this way is a j. n.d. of 2.08″ or a distance of 0.152 μ on the retina.

Langlands'[6] (1929) test-objects were a maltese cross, a square, and a circle. These objects were placed before and behind the plane of a ship's mast and funnels which served as the plane of reference. *L*'s method of deriving his 50%-threshold was the same as Howard's and is subject to the same criticism, for the correctness of the reports has definite reference to the instrumental zero. By the use of lighted circular tubes and an exposure of 4 sec. *L* obtained a liminal value of 2.7″ parallactic difference. He finds that binocular resolution increases with the intensity of the illumination and with interpupillary displacement. From this fact he infers that the size of the retinal image, the effect of diffusion-circles, and the differences of perspective in direct and indirect vision are all negligible. He also attempted to control the factor of size of retinal image, but was unsuccessful in finding a mechanism which would keep the visual angle constant.

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[4] H. J. Howard, A test for judgment of distance, *Amer. J. Ophthal.*, 11, 1919, 656.
[5] E. E. Andersen and F. W. Weymouth, Visual perception and retinal mosaic, *Amer. J. Physiol.*, 64, 1923, 561.
[6] N. M. S. Langlands, Experiments on binocular vision, *Med. Res. Council, Privy Council*, Spec. Rep. Ser. No. 133, 1929.

The Problem

Thus far we have considered only those changes in the visual angle of an object which take place when the object recedes from or approaches the eye. A second change lies in the perspective effect. Thus parallel lines or objects seem, as they recede, to come closer together and this change also gives a cue to the distance of the object. The effect of these two sources of change is the chief problem of this investigation.

An accessory problem concerns the ability to discriminate depth-differences when the eyes are not fixated upon the median plane in the primary position. The question is simply one of relative torsion, assuming that torsion of the eyeball does really occur with change of position. The answer to this question should be instructive, since, if comparable values are found, they would indicate that this type of discrimination is not dependent upon any fixed alignment or arrangement of the retinal elements.

Apparatus

Andersen and Weymouth used a thread-length of 2 cm. They found empirically that this length gave the best results, but they did not vary the diameter of the thread. In a preliminary experiment I determined that slightly better results could be obtained with a thread-diameter of 0.3 mm. instead of the 0.5 mm. which they used. I then designed slits, to be illuminated from behind, that would have minimal dimensions of 2 cm. by 0.3 mm. and whose proportions would remain constant whatever the actual size. The slits were designed to eliminate the factor of visual angle in all the respects mentioned above. As the distance from *S* increased, the actual size of the slit also increased, so that the visual angle remained the same at all positions. In Fig. 1, which illustrates the exposure-slits in detail, the mechanism is shown for the automatic adjustment of the size of the slits. The heavy line A is the length of the slit that was illuminated. Its extent is from the overlapping flange above A to the cross-piece just below. The outside edges of each slit and the inside edges of each frame were milled to an angle of 26′ to allow for proportional changes of the slit-width as the height increased. Small springs extending between the two halves of each slit at the top and bottom held them firmly against the sides of the frame and insured the full width that corresponded with a particular height.

A wedge B is the means of pushing the slit A upward and so increasing the actual size. The movement of this wedge is controlled by that of the attached wheel C on the track D. This track was an inverted brass T-beam, whose distance from the tracks (G) on which the slits moved decreased the greater the distance from *S*. The rear end of this brass T-track was, then, 3 cm. higher than the front position, and consequently the slits were 3 cm. longer at that farthest position. At this rear position the brass T-track was hinged to allow adjustment of the slant when desired. Consequently the farther back the slits were pushed, the greater would be the size of the slit in both directions, because of the upward push of the wheel-wedge mechanism C-B rolling on the sloping track D. The sides of the slits were held firmly down on the wedge by the set of springs indicated by F. The position of the vernier attachment is at E. The

distance between the slits was 38 mm. when the absolute distance from *S* was 207 cm.

The effect of perspective was eliminated by having the tracks on which the slits moved diverge sufficiently, as they receded, to make the slits appear equidistant from each other whatever the position. *S*'s head was held in position by a mouthpiece calibrated in degrees to give accurate and quick adjustment of angular changes in the horizontal direction. The end of the mouthpiece was coated with sealing wax,

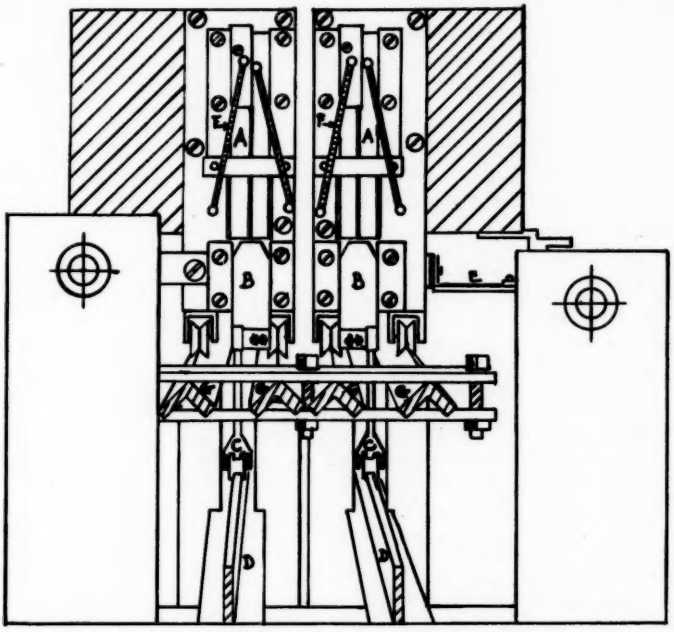

FIG. 1. DETAIL OF EXPOSURE SLITS

and *S* was told to bite into it while it was soft, making an impression of his teeth. This impression when hard held the head immovably. The mouthpiece was attached to a universal joint to allow adjustment in all directions and was clamped to an arm attached to the front support of the apparatus.

The light source was a neon tube behind each slit, the intensity of which was reduced and better diffusion obtained by gluing a piece of ground glass and two thicknesses of white paper just behind each slit. These neon tubes were coiled into a square and attached to a frame fitting into the tin boxes, to which the slits were attached. The electrical connections from the electrodes led out of the back of these tin boxes and thence either to the other box or to the transformer. The exposure apparatus was an adaptation of a Meumann time-sense apparatus to give contacts for illuminating the slits and warning signals at known intervals. The warning light allowed *S* to relax in the chair between exposures.

Although the experiment took place in a dark room *S* was kept light-adapted by a screen illuminated and tinted as nearly as possible the color and intensity of the slits. This was to avoid any possibility of a different light-adaptation for white and for colored lights. One *S*, who was practicing before the screen was tinted, reported that it was much easier to judge the depth-difference after the color was added. The intensity on this screen was controlled by means of a rheostat in series with the screen lights. A second screen of dull black finish was placed just in front of the slits to cut out any reflection from the tin boxes. Both screens had holes in them through which *S* examined the slits.

The readings were taken from a 3-meter steel tape calibrated in mm. and stretched from one end of the track to the other. The left slit was the one varied in position. It had a vernier attached which slid over the tape allowing for readings in either direction as fine as 0.1 mm.

Every effort was made to have everything except the apparatus comparable with the work of Andersen and Weymouth. The exposure-time was the same (1.5 sec.), as was also the statistical method. The factor of irradiation was virtually eliminated by altering the intensity of the light on the adaptation-screen. The interpupillary distance was measured by a mirror type of interpupillary distance-gauge, and each *S* was measured twice at different times by this method.

Method

A practice-period of 5–6 hrs. was given each *S*, in order to acquaint him with the process of judging depth-difference, and also to bring the practice-effect to a minimum. During this practice-period trials were made by the method of minimal changes away from and toward zero in either direction. Thus the general position of subjective zero was found and used as a point of reference for the placing of positive and negative intervals. After the practice-period the method used was that of constant stimuli, the first interval being some distance on either side of the subjective zero. Twenty-five readings were taken at each position, with the magnitude of the interval decreasing to zero. When *S* gave a majority of equal judgments, the interval was increased and 25 readings were taken at the same positions as before, but with an increasing size-interval. In this way the errors of habituation were counterbalanced. The order of presentation, arranged in advance, permitted 200 judgments without repetition.

The *S*s were instructed to record a + for a judgment of farther, = for indecision, and — for nearer. A maximum of 25 judgments was made at a sitting. (With 40 readings between rest-periods, the number of errors in the second 20 increased by as much as 50% over those of the first half.)

The exposure-timing apparatus was going continuously; and all changes of position were made in the dark with the aid of a flashlight. While these changes were being made a small hinged screen was flapped up in front of the second (black) screen to prevent the *S* from seeing the nature of the change.

The formula used for the derivation of the parallactic difference is that found in Helmholtz,[7] Hofmann,[8] and Howard.[9] It is: $p = 2a\delta/E^2$, where p = parallactic value in radians, 2a = interpupillary distance, δ = depth difference, and E = the mean

[7] H. Helmholtz, *Physiological Optics*, (J. P. C. Southall, trans.), 3, 1924, Note on Sec. 30 by Von Kries.
[8] F. B. Hofmann, *Physiologische Optik*, 1925, 418.
[9] H. J. Howard, *loc. cit.*

distance from the subject to the exposure slits. The derivation of this formula can be found in any of the references mentioned. It is understood, of course, that this formula holds only under certain conditions and is not always applicable. The conditions under which it can be used are: (1) the objects lie near the median plane of *S*, (2) the difference in distance between the objects is small, and (3) the distance from *S* to the objects is relatively great.

In order to change from a parallactic value in radians to one in angular seconds, the value in radians is multiplied by $180/\pi$, and that product in turn by 3600. Conversion from angular measure to linear distance on the retina is through multiplication of the tangent of the angle by the distance from the nodal point to the retina (15.5 mm.).

TABLE I

FREQUENCY OF THE THREE TYPES OF JUDGMENT AT THE VARIOUS INTERVALS

E.G.-VIII:SERIES A

Age: 38 I.P.D.: 64 mm.
V.A.: R 20/15 M.B. Exo-1½
L 20/15

Interval (mm.)	Judgments		
	near	equal	far
12		3	47
10	5	5	40
9	6	6	38
8	9	10	31
6	26	17	7
5	38	6	6
4	41	4	5
2	49	1	
Frequency Curve	Far limen:		.80″
	Near limen:		.67″
Parallactic Values			
Angular	Far limen:		2.23″
	Near limen:		2.03″
Linear	Far limen:		.167 μ
	Near limen:		.154 μ

It was decided to use the graphic statistical method of drawing a smooth curve through the points representing the frequency of a certain type of judgment. The point of crossing of the near- and far-curves was taken as the null position or the subjective zero. The limen was defined as the 50% point on the near- and far-curves. This point was derived by averaging the intervals corresponding to the 50%-point, the 75%-point, and the 25%-point. The figure (interval) represented by this average value was then computed from the null position to get the liminal value. In some cases the value of the mean of the judgments, as represented by the value T in the formula for the normal curve, was found. It showed, however, considerable variation and was not calculated for all cases. Several reasons for the use of the frequency-curve instead of the calculated mean are evident. In the first place, the central tendency of the judgments was what was desired, since the absolute numerical values were of no significance. The values derived were to be used only in comparison with the results under similar conditions when the size of the retinal image was not con-

trolled. We have taken a table on a thread-length of 2 cm. from the results of Andersen and Weymouth, and placed it with series A, as the most comparable with our own results. A more cogent reason for not using the calculated mean value is the fact that the individual curves show no regular progression, which is required for the use of such methods. In the sample curve which follows it is

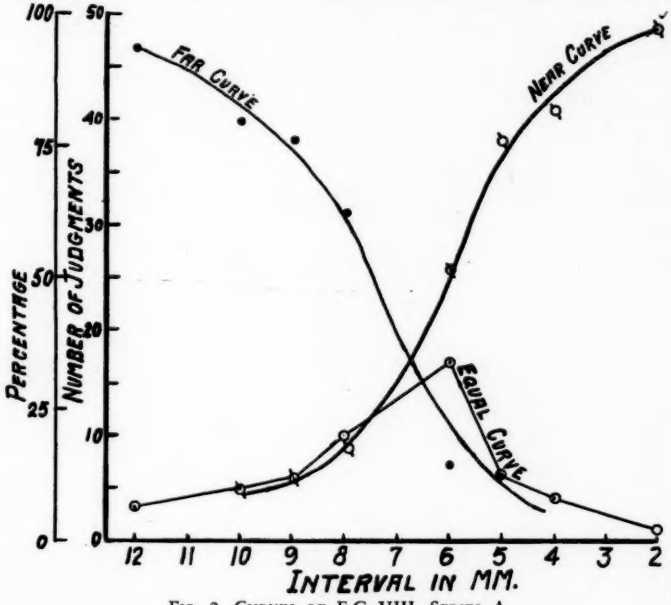

FIG. 2. CURVES OF E.G.-VIII, SERIES A

Far Limen = 0.8 mm.; Near limen = 0.67 mm.; Null position = 6.8 mm.

noteworthy that the tails of the curves, which express a low frequency of that type of judgment, do not come down immediately to the base line after passing the null position. This is indicative of a certain amount of oscillation of judgment, and is partly due, I am sure, to the attitude of *S* just prior to the exposure. If *S* is anticipating a far-judgment, the object will look far, and the converse likewise, no matter what the relative position of the slits. More than this, there is a certain amount of natural fluctuation which has been found to occur when small differences were under consideration.

A sample table and the curve which was derived from it are reproduced. Table I gives the frequency of the three types of judgments at the various intervals. The curve (Fig. 2) is merely a graphic representation of this same frequency, with the interval plotted against the frequency of a certain kind of judgment. Individual tables and curves were made out for each *S* and for each series. Then these individual values were averaged in the summary-tables which are given here.

In the tables, I.P.D. = interpupillary distance, V.A. = visual acuity, F = far-limen, and N = near-limen, M.B. = muscle balance.

Results

Series A. The slits were placed 207 cm. from *S*. Fifty readings were taken at each of 8 positions with the eyes in the primary position. The results of each *S* have been plotted in a curve for each series, and thereafter and only thereafter have summary tables been made out. It is not necessary to give all the individual tables and curves, since the sample given above illustrates very well all the factors which we shall discuss.

Below is given the summary of this series, with a summary of the results of Andersen and Weymouth on a thread-length of 2 cm. (Series B of their investigation).

TABLE II

Summary of Series A

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic value	
						angular	linear
K.D.	I	20	64.5	20/15	F: .59	1.82″	.136μ
				20/15	N: .47	1.45″	.110μ
S.E.	III	19	58	20/15	F: .64	1.81″	.135μ
				20/15	N: .66	1.83″	.139μ
F.R.	V	20	64	20/15	F: .56	1.72″	.129μ
				20/15	N: .40	1.23″	.092μ
V.G.	VI	18	61	20/15	F: .50	1.46″	.110μ
				20/15	N: .60	1.75″	.131μ
J.M.	VII	20	70	20/30	F: .73	2.42″	.183μ
				20/30	N: .57	1.91″	.144μ
E.G.	VIII	38	64	20/15	F: .80	2.23″	.167μ
				20/15	N: .67	2.05″	.154μ
H.W.	X	43	64	20/15	F: 1.03	3.21″	.242μ
				20/15	N: 1.50	4.10″	.340μ
					Av.:	2.12″	.159μ
Andersen and Weymouth:							
Series B			64 mm.	20/15	F: .67	2.16″	.162μ
(Length of thread: 2 cm.				20/15	N: .617	2.00″	.150μ
Separation of threads: 4 cm.)					Av.:	2.08″	.156μ

From Table II it can be seen that the average results of the group of seven *S*s was 2.12″ or 0.159 μ. There is a certain amount of variability in the individual results which does not show in the average. The averaging of the individual results is possible when these values are given in terms of angular seconds, since, at that stage, all individual differences have been already taken into account.

By reference to Table II it can be seen that the average of the results

of Andersen and Weymouth give a value of 2.08″ or 0.156 μ distance on the retina. The difference in magnitude between our results and theirs is sufficiently small to be ignored when the exactness of the two methods is taken into account. One would expect to find some difference between the two sets of results on the basis of the difference in apparatus and technique. We have, however, very definitely found values of the same general magnitude, and can draw comparisons quite adequately and justifiably. We may say, then, that, from the above comparison of results found when the visual angle was and was not under control, such changes are not a necessary cue to fine depth-discrimination; but when present they are likely used as an accessory means.

Series B-1. This is the control-group for the observations with the eyes in secondary positions. There were eight *Ss*, each of whom gave 50 judgments at each of the 8 positions. Since this was a new group of *Ss*, a five-hour practice-period preceded the recording of the judgments. The distance from the *S* to the slits was, in this case, 414 cm.

TABLE III

SUMMARY OF SERIES B-1

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values	
						angular	linear
M.K.	XI	22	60	20/20	F: 2.8	2.1″	.158μ
				20/20	N: 2.8	2.1″	.158μ
J.M.	XII	20	60	20/20	F: 2.5	1.81″	.136μ
				20/20	N: 2.9	2.13″	.160μ
M.E.	XIII	20	62	20/30	F: 3.7	2.78″	.208μ
				20/30	N: 2.9	2.13″	.160μ
H.H.	XIV	21	63	20/15	F: 3.6	2.72″	.204μ
				20/15	N: 4.3	3.23″	.242μ
R.P.	XV	24	66	20/15	F: 1.1	0.87″	.065μ
				20/15	N: 1.0	0.80″	.060μ
E.H.	XVI	19	60	20/15	F: 1.6	1.16″	.087μ
				20/15	N: 2.4	1.73″	.130μ
E.K.	XVII	20	58	20/15	F: 4.5	3.12″	.234μ
				20/15	N: 3.1	2.16″	.162μ
					Av.:	2.11″	.158μ

Table III shows an average value of 2.11″, which corresponds to a retinal distance of 0.158 μ. These results are derived from the first seven *Ss*. The eighth *S* was, on her own admission, discriminating on the basis of "real near" and "not so near." This indicates clearly that her range had not as yet been found at all. In Series B-2 and B-3 her range was discovered and small consistent results were obtained.

Series B-2. The same *Ss* were used as in B-1; the slits were in the same

position as before, and the number of judgments continued at 50 per station.

There was, however, one important change which must be kept in mind. The mouthpiece, which held *S*'s head, was turned 15 degrees to one side or the other. Approximately half of the *S*s had their heads turned to the right and the rest to the left. This meant, of course, that the head turned about a point which was forward of the plane through the two nodal points. The axis of this rotation was 41 mm. When the head was in this position the eyes were turned back to look directly at the slits as before. If the head had been turned about a point directly under the center of the plane through the two nodal points, the eyes would have been turned exactly 15 degrees from the 'primary' position. Since this turning is done with the head at the end of a radius of 41 mm. we must calculate what is the actual turn of the eyes in order to face the slits as before. The radius of this distance moves the head to the side 1 cm., which causes a reduction in the angle of the eyes from the primary position of 8′. Thus the eyes now turn 14° 52′ instead of the full 15°. To find the decrease of the interpupillary distance which corresponds to such turning of the eyes we must take the cosine of 14° 52′ times the original interpupillary distance. The cosine of this angle is 0.9665. I have, however, used only the first two places of this figure in the following computations.

Since the time-allotment was short, it was impossible to give each *S* the length of practice-period that was desirable in this new situation. Consequently the number of mistakes in the early series of readings was probably larger than it would otherwise have been. The parallactic values derived show this when compared with Series B-3.

TABLE IV

SUMMARY OF SERIES B-2

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values	
						angular	linear
M.E.	XIII	20	62	20/30	F: 4.6	3.28″	.246μ
				20/30	N: 3.8	2.70″	.208μ
H.H.	XIV	21	63	20/15	F: 4.7	3.39″	.254μ
				20/15	N: 4.4	3.14″	.234μ
R.P.	XV	24	66	20/15	F: 2.9	2.30″	.173μ
				20/15	N: 2.1	1.60″	.130μ
E.H.	XVI	19	60	20/15	F: 2.6	1.80″	.134μ
				20/15	N: 4.2	2.98″	.223μ
E.K.	XVII	20	58	20/15	F: 4.5	3.00″	.224μ
				20/15	N: 4.2	2.80″	.210μ
F.L.	XVIII	17	61	20/20	F: 2.3	1.69″	.122μ
				20/20	N: 2.5	1.86″	.132μ
					Av.:	2.54″	.190μ

The figures in Table IV give the average result for 6 *S*s. This average was found better for purposes of comparison than the individual readings since there is a certain amount of variability which cannot be eradicated in the number of judgments used. In the computation of the average

values given for this series the results of M.K.-XI and J.M.-XII were not used. The reason in the case of M.K.-XI was that he consistently refused to give any equal judgments no matter how small the interval. It was learned in the case of J.M.-XII that after the first series a guess would be made one way or the other when it was impossible to tell the relative position.

TABLE V

SUMMARY OF SERIES B-3

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values	
						angular	linear
M.E.	XIII	20	62	20/30	F: 3.3	2.32″	.175 μ
				20/30	N: 4.1	2.90″	.217 μ
H.H.	XIV	21	63	20/15	F: 2.7	1.96″	.147 μ
				20/15	N: 2.3	1.67″	.124 μ
R.P.	XV	24	66	20/15	F: 1.7	1.30″	.097 μ
				20/15	N: 2.2	1.67″	.125 μ
E.H.	XVI	19	60	20/15	F: 1.8	1.25″	.095 μ
				20/15	N: 3.2	2.22″	.166 μ
F.L.	XVIII	17	61	20/20	F: 3.1	2.17″	.163 μ
				20/20	N: 3.0	2.11″	.158 μ
					Av.:	1.96″	.147 μ

We find that the average value for this series in angular seconds is 2.54″, which corresponds to the 0.190 μ linear distance to the retina. These values are slightly larger than those found for Series B-1. Some increase would be expected because of the lack of the full training period. Otherwise the figures should coincide, since the shortened interpupillary distance has already been taken into account in calculating the size of the parallactic angle.

The question which instantly arises is: Can the increase in the parallactic values be explained on the basis of lack of practice or is there some other factor to consider? A second possibility would be the factor of torsion, which is supposed to be present when the eyes are turned to the side. It is very clear that we have here a confusion due to the presence of two factors. Let us look for the answer to this question in the results of Series B-3, where we find merely the addition of a certain amount of torsion.

Series B-3. In this final series of our experiment we have the same situation found in B-2 with a slight addition.

The addition is the elevation by 15 degrees of the rear end of the apparatus. This, it is clear, increases the torsion considerably when the head is kept turned

15° to the side. The approximate amount of torsion of the eyes in this final position is equal to 1° 59′. An additional factor in this series is the fact that when the rear of the apparatus is elevated the absolute distance to the slits decreases slightly. The reason for this is that the mouthpiece is so situated that the eyes are 17 cm. above the front support of the apparatus and remain stationary in position. Thus the mouthpiece does not move when the apparatus is elevated, resulting in a slight decrease in distance from *S* to the slits. This decrease amounted to 4 cm., making the distance 410 cm. The difference in the parallactic angles due to this difference is almost negligible, amounting to 0.02″ or 0.00155 μ.

Here also the results of M.K.-XI and J.M.-XII were eliminated before the averages were calculated for the same reasons as stated in Series B-2. The subject E.K.-XVII did not finish this series and no results are included for him.

In this summary of this final series we have results which are slightly lower than those found in Series B-2. The average for B-2 was 2.41″, corresponding to 0.180 μ, while in B-3 we have obtained an average value of 2.30″ which corresponds to a linear distance on the retina of 0.173 μ. The decrease in the values for this last series is due, as we assume, to the loss from the lack of practice already mentioned in B-2. Since there is no increase in B-3 over B-2, we may safely conclude that in human adults any torsion that may be present does not reduce the fineness of binocular perception of depth differences.

Conclusions

As we have already suggested in the discussion of Series A, the values derived when the factor of the visual angle was kept constant are very little different from those calculated from the results of Andersen and Weymouth. These results are given at the end of the first series. The difference between our average value and theirs is 0.04″ or 0.003 μ. This is too small a value to be significant in a study of this sort. We may safely conclude, then, that normal changes in the visual angle of an object are not necessary cues in the binocular discrimination of depth-differences, although they are probably used as accessory cues when present. It would be expected that there would be some difference between the two sets of results due to the fact that in our investigation a different light-source and slightly different test-objects were used.

The comparison of the values derived from the three Series, B-1, B-2, and B-3, shows the relationship between the results with the eyes in the 'primary' position and those found when they were in 'secondary' positions. By reference to the summaries of these three series we can compare directly the results found under the various conditions. By means of such a comparison we find that in B-1, when the eyes were directed straight ahead, the average value of all subjects was 2.11″ which corresponds to a linear

distance on the retina of 0.158 μ. As has been already stated there was an insufficient practice-period before the readings were taken on B-2 with the eyes 15° to the side. The average values found for this series were 2.54″ or a retinal distance of 0.190 μ. This is an increase of 0.43″ and (0.032 μ) over B-1. As we have noted above, there is a confusion in the results of this series on account of the effect of the lack of practice and the possible effect of torsion in the eye-movement. In Series B-3 we have only an increase of this possible factor of torsion. The values for this last series, when the apparatus was elevated, are 1.96″ and 0.147 μ. If the results of the second series are compared with the last, it will be evident that there is a slight decrease in the values obtained. This decrease, which was found with a considerable increase in torsion (if real torsion does occur) would seem to show that the changing of the position of the eyes to this extent does not increase the limen more than would be expected on account of the decrease of the interpupillary distance. If there is real torsion of the eyeball in moving from one position to another, it does not affect the fineness of binocular depth-discrimination. It is evident, moreover, that there is no necessary relationship between this type of discrimination and any fixed alignment or arrangement of the retinal elements.

Supplementary Note. It will be noticed, if the reader turns to Table I and curve appended (Fig. 2), that there is a shift in the position of the equal judgments away from the instrumental zero. In 5 of the *Ss* this shift was discovered, and in some cases to a considerable extent. When such a shift does occur it is impossible to use the correctness of the judgment as the basis for deriving the limen. Unfortunately this principle of correctness has been used by some of the experimental workers, notably Howard and Langlands, as mentioned above. It is, however, quite conceivable that the poor results recorded for Howard's *Ss*, who had somewhat anomalous vision, might have been due to this reference to the instrumental zero. Such a method as he uses starts with an unproved assumption, instead of basing the derivation entirely upon *Ss'* reports. A reasonable explana-

Subject	Shift	No.	V.A.	Muscle-balance
E.G.	backwards	VIII	20/15 20/15	Exo—1½, Hyperphoria—½ Abduction—6, Adduction—8
M.E.	forwards	XIII	20/30 20/30	Abduction—6 Adduction—0
E.H.	backwards	XVI	20/15 20/15	Eso—2, Abduction—6 Adduction—12.
H.H.	forwards	XIV	20/15 20/15	Accommod. Exo—2, Abduction—6 Adduction—10
F.L.	backwards	XVIII	20/20 20/20	O.K. O.K.

tion of such cases may be found in an examination of the refraction-records of those *Ss* whose results show such a shift. The tabulation above gives in detail the visual acuities and the muscle-balance of these *Ss*.

It will be promptly noted that there is very little asymmetry in the acuity of the two eyes. When we look at the muscle-balance of each subject we do find some discrepancy. It seems that in all but one case (F.L.) there is imbalance of the eye-muscles with a shift in either direction. We may suggest, then, that a lack of balance of the eye-muscles is often accompanied by such a shift in the position of equal judgments in front of or behind the instrumental zero.[10]

[10] It may be questioned how well the apparatus described above controlled the size of the visual angle. One *S*, whose results are not included in Series A, worked for 30 hours without being able to decrease the number of errors below 8 or 10 out of 20 readings, with a depth interval of 3 mm. When the work was practically done and he was asked what he had used as his basis of discrimination, he reported that he had tried to judge on the basis of the size of the slit. The nearer slit should be larger, he thought. It seems to the writer that this is a fair proof that *accurate* discrimination on the basis of the apparent size of the slit was impossible.

THE STATISTICS OF THE METHOD OF CORRECT MATCHINGS

By Dwight W. Chapman, Harvard University

Several recent studies[1] have employed what may be termed the *method of correct matchings*. Allport and Vernon, for example, report experiments whose purpose was to determine whether certain aspects of personality are conveyed in handwriting. The method is to require *Ss* to attempt to match character-sketches of a group of persons with specimens of the handwriting of those same persons. The number of correct matchings thus achieved is then compared with the number to be expected from chance alone, and any significant excess of the former over the latter constitutes evidence that real similarities between sketches and handwriting must govern the *Ss*' performance.

This experimental method is at once simple and direct, and capable of extension to the solution of many analogous problems. However, its whole effectiveness rests obviously upon a correct understanding of what is to be expected from chance matchings; and certain phases of this statistical problem have, so far as I know, not yet been discussed for the psychologist. Mathematical mistakes in the use of the method have already occurred, as we shall see; and in critical cases they may easily lead to a quite false assessment of the data. This paper will therefore treat the following questions in order:

(1) Given two series of t elements each, and defining a correct matching as the correct pairing of one element in the first series with its companion in the second series, what is the probability that a single chance arrangement of the two series will result in *exactly* s correct matchings?

(2) What is the probability that a single chance arrangement of the two series will result in *at least* s correct matchings?

(3) If n independent chance arrangements of the two series are made, what is the probability that the mean number of correct matchings for the n

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[1] G. W. Allport and P. E. Vernon, *Studies in Expressive Movement*, 1933, 212–242; F. H. Allport, Written composition and characteristics of personality, in *Outline of an Inquiry Now Being Made at Syracuse University, etc.*, 1933, 33–42; H. Cantril and H. A. Rand, Additional study of the determination of personal interests by psychological and graphological methods, to appear in *Character and Personality*, 2, 1934.

arrangements will be at least $\bar{s}$? (That is, how good may we expect the average of the chance performances of n subjects to be?)

(1) *The probability that a single arrangement of two series of t elements will give exactly s correct matchings.* Let a series of t x-elements, $x_1, x_2, \cdots, x_t$, be held in some given order, and consider all the possible ways in which a series of t y-elements, $y_1, y_2, \cdots, y_t$, can be arranged against it. In what proportion of these ways will there result exactly s correct matchings between members of the two series?

Since we wish the general expression for the number of permutations of t elements which will give us exactly s correct matchings, we begin by putting s of the y elements in the same position as the corresponding s x-elements, and we shall hold them in this position throughout the following discussion. This leaves $(t-s)$ elements, which can be arranged in $(t-s)!$ permutations. Therefore our first term in the solution is

$$(t-s)! \quad \text{[i]}$$

But this number is too large; for some of these $(t-s)!$ permutations are such that at least one of the elements in them is correctly matched with an element in the x-series. We must therefore subtract the number of such cases, since we wish only the original s elements to match. The number to be subtracted can be determined by making one of the $(t-s)$ elements match, which leaves $(t-s-1)$ elements, capable of $(t-s-1)!$ permutations. Since, however, the one matching element can be chosen in $(t-s)$ ways from amongst the $(t-s)$, and since we have $(t-s-1)!$ permutations of the remaining elements for each such choice, the number of permutations of $(t-s)$ elements such that at least one element matches is the product of these two expressions. We therefore subtract $(t-s-1)!\ (t-s)=(t-s)!$ from (i) above, giving as the first two terms of the answer

$$(t-s)! - (t-s)! \quad \text{[ii]}$$

This number is too small, for the following reason: Suppose the element held constant in the above operation were y_i. Then the number of permutations of the remaining $(t-s-1)$ elements includes permutations in which also the jth (let us say) element matches. But we have just subtracted such a permutation as this twice over—once for the matching of y_i and once again for the matching of y_j. To correct this error, we must restore all permutations of the $(t-s)$ elements in which at least two elements match. Therefore we now hold two of the $(t-s)$ elements matched, which can be done in $(t-s)!/(t-s-2)!2!$ ways, and for each way there are $(t-s-2)!$ permutations of the remaining elements. Consequently we add to (ii) the

product $[(t-s)!/(t-s-2)!2!]\ (t-s-2)!=(t-s)!/2!$, giving as the first three terms of the answer

$$(t-s)!-(t-s)!+[(t-s)!/2!]\ldots\ldots\ldots\ldots\ldots[\text{iii}]$$

By analogy with the last step, we must now subtract the number of permutations containing at least three matchings, since this last term has restored the number of such cases twice over. If we continue these corrections, alternately adding and subtracting a term, we shall get the alternating convergent series

$$\frac{(t-s)!}{0!}-\frac{(t-s)!}{1!}+\frac{(t-s)!}{2!}-\frac{(t-s)!}{3!}+\frac{(t-s)!}{4!}\cdots,$$

which is the first terms of the expansion of $1/e$. The last term will be $(-1)^{t-s}(t-s)!/(t-s)!$, where the factor $(-1)^{t-s}$ indicates that the sign of the term will be positive if $(t-s)$ is even and negative if $(t-s)$ is odd.

But this whole series must, finally, be multiplied by $t!/(t-s)!s!$, which is the number of ways in which we might have chosen the original s elements which we have held constant throughout the discussion. The *number* of permutations of t elements in which exactly s of the elements match s elements in a standard series is therefore given by

$$\frac{t!}{(t-s)!s!}\left[\frac{(t-s)!}{0!}-\frac{(t-s)!}{1!}+\frac{(t-s)!}{2!}-\cdots+(-1)^{t-s}\frac{(t-s)!}{(t-s)!}\right],$$

which reduces to[2]

$$\frac{t!}{s!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\cdots+(-1)^{t-s}\frac{1}{(t-s)!}\right]\ldots\ldots\ldots[1]$$

If (as is generally more to the point) we wish to know the *proportion* of arrangements which will give exactly s matchings—in other words, the probability that a single arrangement will give s matchings—the expression (1) must be divided by the total possible number of arrangements of t things, which is $t!$ and which gives

[2] I have been shown by Dr. Hassler Whitney of the Harvard Department of Mathematics that the expression following can be directly derived from an expansion in symbolic logic. The present demonstration is probably clearer to the non-mathematician, though inductively less satisfactory.

$$\frac{1}{s!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\cdots+(-1)^{t-s}\frac{1}{(t-s)!}\right]\ldots\ldots\ldots[2]$$

I have evaluated this last expression to four decimal places for all values of t and s; the results are given in Table I. The probabilities associated with any value of s converge so rapidly that actual calculation beyond $t=9$ is unnecessary.

TABLE I

THE PROBABILITY OF OBTAINING EXACTLY s CORRECT MATCHINGS WHEN t THINGS ARE ARRANGED

t	$s\to 0$	1	2	3	4	5	6	7	8	9	10
1	.0000	1.0000									
2	.5000	.0000	.5000								
3	.3333	.5000	.0000	.1667							
4	.3750	.3333	.2500	.0000	.0417						
5	.3667	.3750	.1667	.0833	.0000	.0083					
6	.3681	.3667	.1875	.0556	.0208	.0000	.0014				
7	.3679	.3681	.1833	.0625	.0139	.0042	.0000	.0002			All further
8	.3679	.3679	.1840	.0611	.0156	.0028	.0007	.0000	.0000		terms approx-
$t\geqq 9$	.3679	.3679	.1839	.0613	.0153	.0031	.0005	.0001	.0000		imate .0000

(2) *The probability that a single arrangement of t elements will give s or more correct matchings.* We cannot argue directly from the probabilities in Table I to the significance of an obtained number of correct matchings; for the real test of the significance of an obtained s is the probability of obtaining by chance a value of s as great *or greater*. Now, if $P_s, P_{(s+1)}, \cdots$ are the probabilities respectively of obtaining exactly s, $(s+1), \cdots$ correct matchings, the probability of obtaining s *or more* correct matchings is given by

$$P_{(s)}+P_{(s+1)}+\cdots+P_{(t)}.$$

Hence, if Table I is summed from right to left, we have Table II, which shows the probability of obtaining s *or more* correct matchings from a single arrangement of t elements.

As an example of the use of this table, let us consider an actual experimental case. In Power's study of handwriting (reported by Allport and Vernon), one graphologist correctly matched 5 out of 10 scripts with an equal number of character sketches. What is the significance of this performance? From Table II, opposite $t=10$ and $s=5$, we find that as good a result as this or better should be expected by chance only 37 times in 10,000.

(3) *The significance of the mean number of correct matchings as the result of n independent trials.*[3] Suppose that each of n Ss attempts to arrange a

[3] I wish to thank Professor E. V. Huntington of the Harvard Department of Mathematics for a number of very helpful suggestions on problems involved in this section.

series of t elements against a second such series. The first S produces s_1 correct matchings, the second S produces s_2 correct matchings, etc. If we let $\bar{s}$ represent the mean number of correct matchings for the n Ss,

$$\bar{s} = \frac{s_1 + s_2 + \cdots + s_n}{n},$$

what will be the probability of obtaining a given value of $\bar{s}$? We can answer this question if we can describe the distribution of the means of samples

TABLE II

THE PROBABILITY OF OBTAINING s OR MORE CORRECT MATCHINGS WHEN t THINGS ARE ARRANGED

$s\to$ / t	0	1	2	3	4	5	6	7	8	9	10
1	1.0000	1.0000									
2	1.0000	.5000	.5000								
3	1.0000	.6667	.1667	.1667							
4	1.0000	.6250	.2917	.0417	.0417						
5	1.0000	.6333	.2583	.0916	.0083	.0083					
6	1.0000	.6319	.2652	.0777	.0221	.0014	.0014				
7	1.0000	.6321	.2641	.0808	.0183	.0044	.0002	.0002		All further	
8	1.0000	.6321	.2642	.0802	.0191	.0035	.0007	.0000	.0000	terms approx-	
$t\geqq 9$	1.0000	.6321	.2642	.0803	.0190	.0037	.0006	.0001	.0000	imate .0000	

made up of n s's each, taken from a universe where the probability of drawing an s of a given value is the probability shown in Table I opposite that s and the proper t.

Let us consider first the distribution for $t=5$, and then generalize from it. If we tabulate this distribution in terms of Equation [2], we have the equations of Table III.

Now the mean of any distribution is given by the sum of the products of its items by their relative frequencies. Hence, multiplying the values of s (Table III) by their relative frequencies, and remembering that $1/1!=1/0!$, $2/2!=1/1!$, or in general, $a/a!=1/(a-1)!$, we have the terms shown in Table IV. Consider the sum of these terms as the sum of the diagonals which they form, from lower left to upper right. Excepting only the diagonal containing the single term $1/0!0!$ $(=1)$, every diagonal is of the general form

$$\frac{1}{m!0!} - \frac{1}{(m-1)!1!} + \frac{1}{(m-2)!2!} - \cdots + (-1)^{m-1}\frac{1}{1!(m-1)!} + (-1)^m \frac{1}{0!m!} \quad \ldots\ldots [a]$$

TABLE III

EQUATIONS FOR DISTRIBUTION OF CORRECT MATCHINGS WHEN $t=5$

s	Relative frequency
0	$\frac{1}{0!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}+\frac{1}{4!}-\frac{1}{5!}\right]=\frac{1}{0!0!}-\frac{1}{0!1!}+\frac{1}{0!2!}-\frac{1}{0!3!}+\frac{1}{0!4!}-\frac{1}{0!5!}$
1	$\frac{1}{1!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}+\frac{1}{4!}\right]=\frac{1}{1!0!}-\frac{1}{1!1!}+\frac{1}{1!2!}-\frac{1}{1!3!}+\frac{1}{1!4!}$
2	$\frac{1}{2!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}\right]=\frac{1}{2!0!}-\frac{1}{2!1!}+\frac{1}{2!2!}-\frac{1}{2!3!}$
3	$\frac{1}{3!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}\right]=\frac{1}{3!0!}-\frac{1}{3!1!}+\frac{1}{3!2!}$
4	$\frac{1}{4!}\left[\frac{1}{0!}-\frac{1}{1!}\right]=\frac{1}{4!0!}-\frac{1}{4!1!}$
5	$\frac{1}{5!}\left[\frac{1}{0!}\right]=\frac{1}{5!0!}$

TABLE IV

MEAN OF DISTRIBUTION OF CORRECT MATCHINGS WHEN $t=5$

s	$s\times$(relative frequency)
0	— — — — — —
1	$\frac{1}{0!0!}-\frac{1}{0!1!}+\frac{1}{0!2!}-\frac{1}{0!3!}+\frac{1}{0!4!}$
2	$\frac{1}{1!0!}-\frac{1}{1!1!}+\frac{1}{1!2!}-\frac{1}{1!3!}$
3	$\frac{1}{2!0!}-\frac{1}{2!1!}+\frac{1}{2!2!}$
4	$\frac{1}{3!0!}-\frac{1}{3!1!}$
5	$\frac{1}{4!0!}$

But the sum of the terms in this typical series can be shown to equal 0; for, consider the binomial expansion of $(a-b)^m$:

$$(a-b)^m = {}_mC_0a^m - {}_mC_1a^{m-1}b + {}_mC_2a^{m-2}b^2 - \cdots + (-1)^{m-1}{}_mC_{m-1}ab^{m-1} + (-1)^m{}_mC_mb^m.$$

Setting $a=1$ and $b=1$, and writing the coefficients in terms of factorials, we have

$$(1-1)=\frac{m!}{m!0!}-\frac{m!}{(m-1)!1!}+\frac{m!}{(m-2)!2!}-\cdots+(-1)^{m-1}\frac{m!}{1!(m-1)!}+(-1)^m\frac{m!}{0!m!}.$$

But $(1-1)^m=0^m=0$. Therefore

$$m!\left[\frac{1}{m!0!}-\frac{1}{(m-1)!1!}+\frac{1}{(m-2)!2!}-\cdots+(-1)^{m-1}\frac{1}{1!(m-1)!}+(-1)^m\frac{1}{0!m!}\right]=0,$$

and therefore the expression in brackets, which is the same as [a] above, equals zero. Consequently every diagonal but the first in our tabulation has the value 0, and only the first diagonal, whose value is 1, contributes to the mean. The mean of the distribution is therefore 1.

It will be seen at once that this holds true not only when $t=5$, as in our example, but for any value of t greater than 1; for the effect of increasing or diminishing t is simply to add or subtract a number of diagonals, each of which equals 0.

Analogously, we can determine the moments of the distribution of s's, namely μ_2, μ_3, and μ_4, by investigating the sums of the products: $s^2\times$(relative frequency), $s^3\times$(relative frequency), and $s^4\times$(relative frequency), and correcting these sums for the mean as an origin. The demonstration that diagonals of certain orders in these sums are equal to 0 is more complicated than the demonstration in the case of the mean. But by subtracting from them series like [a] above, and performing certain obvious reductions, the proof can be performed.

From these moments, the measures of skewness and kurtosis, β_1 and β_2, can be calculated, since $\beta_1=\mu_3^2/\mu_2^3$, and $\beta_2=\mu_4/\mu_2^2$. The results are shown, for the various values of t, in Table V.

We have, then, the convenient discovery that these constants become invariable at $t=4$. Since it seems more than likely that values of 4 and higher for t contain all the experimentally useful cases, we shall confine our attention to that range.

We have, therefore, to predict the distribution of mean values of s ($\bar{s}$) for

TABLE V

CONSTANTS OF THE DISTRIBUTION OF s FOR GIVEN VALUES OF t IN TABLE II.

	Constant					
t	Mean	μ_2	μ_3	μ_4	β_1	β_2
1	1	0	0	0	—	—
2	1	1	0	1	0	1
3	1	1	1	3	1	3
$t \geqq 4$	1	1	1	4	1	4

samples of size n drawn from the parent population having the constants in the last row of Table V.

Now it is known that the following relations hold between the constants of a parent distribution (irrespective of its abnormality) and the constants of the distribution of means of all possible samples of size n drawn from the parent population:[4]

$$\text{(a)} \qquad M_{\text{means}} = M_{\text{parent}},$$

$$\text{(b)} \qquad \sigma_{\text{means}} = \frac{\sigma_{\text{parent}}}{\sqrt{n}},$$

$$\text{(c)} \qquad \beta_{1\,\text{means}} = \frac{\beta_{1\,\text{parent}}}{n},$$

$$\text{(d)} \qquad \beta_{2\,\text{means}} = 3 + \frac{\beta_{2\,\text{parent}} - 3}{n}.$$

Applying the values opposite $t \geqq 4$ in Table III to these equations, we have Table VI. The distribution of $\bar{s}$ for any value of n is therefore skewed

TABLE VI

CONSTANTS OF THE DISTRIBUTION OF MEANS ($\bar{s}$) OF SAMPLES OF SIZE n, WHEN $t \geqq 4$.

$$M = 1 \qquad \sigma = \frac{1}{\sqrt{n}} \qquad \beta_2 = \frac{1}{n} \qquad \beta_2 = 3 + \frac{1}{n}$$

($\beta_1 < 1$) and leptokurtic ($\beta_2 > 3$), although these two abnormalities decrease as n becomes infinite.[5] Furthermore, since for any value of n, Pearson's cri-

[4] Cf. B. H. Camp, *The Mathematical Part of Elementary Statistics*, 1931, 245, Theorem II. Camp's $\alpha_3 = \sqrt{\beta_1}$, and his $\alpha_4 = \beta_2$. The useful equations (c) and (d) are curiously infrequent in the common statistical texts.

[5] Hence the probabilities for average results in the table on p. 230 of Allport and Vernon, *op. cit.*, are erroneous. I am unable to deduce the procedure used in that table. The probabilities (P) appear to be the result of interpolation amongst such values as occur in our Table I. If that is so, the method is wrong on three counts: first, such an interpolation gives the probability of obtaining *exactly* s, not s or more, correct matchings, and is therefore of no use as a measure of significance; secondly, such values do not apply anyway to the significance of the mean performance of a group of judges (for this is the problem we are now treating); and,

terion κ is negative, the distribution of $\bar{s}$ can be best represented by his Type I curve. We could therefore determine very closely the significance of any obtained value of $\bar{s}$ by calculating the ordinates of a Type I distribution having the moments given in Table VI (for the appropriate value of n), and by then summing the ordinates at $\bar{s}$ and at all higher values which the mean can take, as a method of approximate quadrature.

But this solution of the problem is in practice inconvenient, since the calculation of a Type I curve is at best irksome, and would have to be repeated for all the values of n involved in any actual problem. Very happily, however, a Type III distribution curve gives an extremely close approximation to the Type I curve within the limits of skewness with which we are concerned;[6] and for the Type III curve we have Salvosa's excellent tables of areas for different deviations from the mean and for different degrees of skewness.[7]

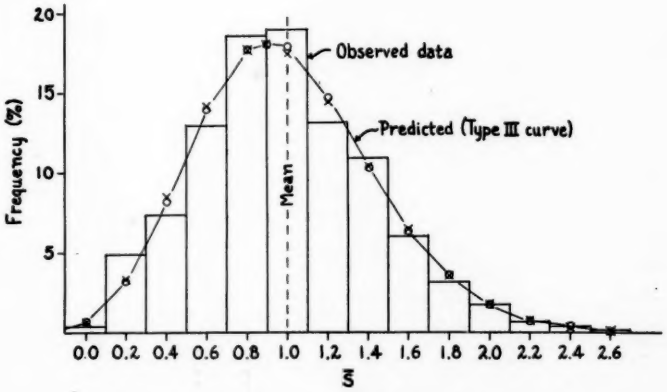

FIG. 1. SHOWING PREDICTED AND OBSERVED FREQUENCIES

Fig. 1 shows the ordinates of the Type I curve (crosses) and of the Type III curve (circles) for the case that $n=5$; and it shows, for comparison, a histogram of the results of the following empirical example: Ten cards, numbered 1 to 10, were shuffled and laid out in a series, and the number of cards matching their position in the series was recorded. This procedure was repeated, by the members of my class in statistics, in all 4,500 times. These 4,500 empirical values

thirdly, the columns following P in Allport and Vernon's table give areas and critical ratios evidently taken from the normal curve, whereas we have just seen that the values of $\bar{s}$ are not normally distributed.

[6] On the fitting of Type I and III curves, see D. C. Jones, *A First Course in Statistics*, 1929, chap. 16.

[7] L. R. Salvosa, Tables of Pearson's Type III function, *Ann. Math. Statist.*, 1, 1930, 191–198. Salvosa's t (the argument) $=x/\sigma$, and the degrees of skewness are values of $\sqrt{\beta_1}$.

of s were then sampled by successive fives, the mean of which was computed, thus affording 900 values of $\bar{s}$. It will be seen that both the Type I and the Type III curves give a reasonable description of the empirical data; and, more important, that the two types are practically identical.

Finally, since Salvosa's Tables may be inaccessible to some, and for the sake of convenience, I present Table VII, which gives the probability (p) of obtaining a mean number of correct matchings as great as $\bar{s}$ or greater from n independent trials (*e.g.* from n judges or Ss) when the number of things to be matched (t) in each series is four or more. The table was calculated by interpolation in Salvosa's Tables. The values of n run by units from 2 to 15, and thence by fives to 30. The argument, $\bar{s}$, can, of course, take only certain values (since the total number of correct matchings for n judges must be an integer); and I have given all these possible values of $\bar{s}$, from 1 (which is the mean chance performance) up to whatever value gives $p = 0.000$ to three places.

TABLE VII

SHOWING THE PROBABILITY (p) THAT THE MEAN NUMBER OF CORRECT MATCHINGS RESULTING FROM n INDEPENDENT TRIALS WILL BE AS GREAT AS $\bar{s}$ OR GREATER, WHEN $t \geqq 4$

$n=2$		$n=3$		$n=4$		$n=5$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.597	1.000	.580	1.000	.568	1.000	.559
1.500	.325	1.333	.354	1.250	.371	1.200	.386
2.000	.143	1.667	.186	1.500	.215	1.400	.134
2.500	.054	2.000	.085	1.750	.111	1.600	.068
3.000	.018	2.333	.035	2.000	.052	1.800	.033
3.500	.005	2.667	.013	2.250	.022	2.000	.015
4.000	.002	3.000	.004	2.500	.009	2.200	.006
4.500	.000	3.333	.001	2.750	.003	2.400	.002
		3.667	.000	3.000	.001	2.600	.001
				3.250	.000	2.800	.000

$n=6$		$n=7$		$n=8$		$n=9$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.553	1.000	.551	1.000	.549	1.000	.546
1.167	.395	1.143	.401	1.125	.408	1.111	.412
1.333	.257	1.286	.270	1.250	.284	1.222	.294
1.500	.153	1.429	.171	1.375	.185	1.333	.198
1.667	.085	1.571	.099	1.500	.112	1.444	.124
1.833	.043	1.714	.054	1.625	.064	1.556	.074
2.000	.021	1.857	.028	1.750	.035	1.667	.042
2.167	.010	2.000	.013	1.875	.018	1.778	.032
2.333	.004	2.143	.006	2.000	.009	1.889	.011
2.500	.001	2.286	.003	2.125	.004	2.000	.006
2.667	.000	2.429	.001	2.250	.002	2.111	.003
		2.571	.000	2.375	.001	2.222	.001+
				2.500	.000	2.333	.001−
						2.444	.000

TABLE VII (CONTINUED)

$n=10$		$n=11$		$n=12$		$n=13$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.543	1.000	.540	1.000	.537	1.000	.536
1.100	.416	1.091	.421	1.083	.426	1.077	.427
1.200	.304	1.182	.312	1.167	.419	1.154	.323
1.300	.209	1.273	.220	1.250	.228	1.231	.237
1.400	.135	1.364	.145	1.333	.156	1.308	.165
1.500	.084	1.455	.092	1.416	.101	1.385	.110
1.600	.049	1.545	.056	1.500	.063	1.462	.070
1.700	.027	1.636	.033	1.583	.038	1.538	.043
1.800	.015	1.727	.018	1.667	.022	1.615	.026
1.900	.008	1.818	.010	1.750	.012	1.692	.014
2.000	.004	1.909	.005	1.833	.006	1.769	.008
2.100	.002	2.000	.002	1.916	.003	1.846	.004
2.200	.001	2.091	.001+	2.000	.002	1.923	.002
2.300	.000	2.128	.001−	2.083	.001	2.000	.001+
		2.273	.000	2.167	.000	2.077	.001−
						2.154	.000

$n=14$		$n=15$		$n=20$		$n=25$		$n=30$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.533	1.000	.534	1.000	.529	1.000	.527	1.000	.524
1.071	.431	1.067	.432	1.050	.445	1.040	.447	1.033	.452
1.143	.331	1.133	.335	1.100	.355	1.080	.371	1.067	.384
1.214	.243	1.200	.251	1.150	.279	1.120	.300	1.100	.314
1.286	.173	1.267	.181	1.200	.213	1.160	.237	1.133	.255
1.357	.118	1.333	.125	1.250	.156	1.200	.182	1.167	.203
1.429	.077	1.400	.083	1.300	.113	1.240	.137	1.200	.158
1.500	.048	1.467	.053	1.350	.079	1.280	.101	1.233	.119
1.571	.029	1.533	.037	1.400	.052	1.320	.072	1.267	.089
1.643	.017	1.600	.020	1.450	.035	1.360	.050	1.300	.065
1.714	.010	1.667	.011	1.500	.022	1.400	.034	1.333	.047
1.786	.005	1.733	.006	1.550	.014	1.440	.023	1.367	.032
1.857	.003	1.800	.003	1.600	.008	1.480	.015	1.400	.022
1.929	.002	1.867	.002	1.650	.005	1.520	.009	1.433	.015
2.000	.001	1.933	.001+	1.700	.003	1.560	.006	1.467	.010
2.071	.000	2.000	.001−	1.750	.002	1.600	.004	1.500	.006
		2.067	.000	1.800	.001	1.640	.002	1.533	.004
				1.850	.000	1.680	.001+	1.567	.002+
						1.720	.001−	1.600	.002−
						1.760	.000	1.633	.001+
								1.667	.001−
								1.700	.000

The following examples will illustrate the use of Table VII.

(1) Eleven judges match six samples of handwriting with six character-sketches. The mean number of correct matchings for the group is 1.727. Under $n=11$, opposite 1.727, we find that so good a performance may be expected by chance only 18 times in 1,000.

(2) Fifteen judges achieve a mean performance of 2.2 correct matchings. Entering the column for $n=15$, we find that $\bar{s}=2.2$ is beyond the range of the table. Therefore this result may be expected less than once in 1,000 chance trials.

(3) Cantril and Rand's group of 26 laymen matched six scripts and six sketches with an average of 1.1 correct matchings. Interpolating between the values of p for $\bar{s}=1.08$ and

$\bar{s} = 1.12$ in column ($n = 25$), and then interpolating between this result and the value of p for $\bar{s} = 1.1$ in column ($n = 30$), we arrive at $p = 0.332$, approximately.

(4) In Power's study, 185 judges achieved a mean matching of 1.834. From Table IV, using $n = 185$, we find for the distribution of such means, $\sigma = .0735$, $\beta_1 = .0054$, and the skewness, $\sqrt{\beta_1} = 0.0735$. The next lower possible value of the mean than 1.834 is 1.827; and the lower boundary of the class interval whose midpoint is 1.834 is therefore halfway between these values, or 1.838. In units of σ, this represents a deviation from the mean of the means (1.0) amounting to 11.4. Entering Salvosa's tables with $t = 11.4$ and skewness $= 0.07$, we find that we are greatly beyond the range of his six-place tabulation. The probability of 1.834 as the mean achievement of 185 judges by chance is therefore far below 1 in 1,000,000 (as against the erroneous value of 0.208 in Allport and Vernon's table)!

JOST'S LAW AND RETROACTIVE INHIBITION

By STEUART H. BRITT, Yale University, and MARION E. BUNCH, Washington University

A series of experiments in the psychology of learning was conducted by Jost in 1897.[1] On the basis of the results he formulated a statement of the relationship between the efficacy of practice and the age of associations, when the associative connections involved are regarded as being equal in functional strength just prior to the introduction of the new practice period. Warren and Carmichael state Jost's conclusions as twofold.[2] "When two associative connections are originally of equal strength but of unequal age, new repetition increases the strength of the earlier more than of the later association." Similarly, the older of the two associations "fades less rapidly than does the newer." These two generalizations, known as Jost's Law, appear fairly frequently in the literature. The limiting conditions of this law, its relation to the problems involved in the economy of distributed as compared with massed effort, and to the problem of retention, have received scant attention.

The experimental work in regard to retroactive inhibition indicates that the amount of retroaction varies with a number of factors; *e.g.* the similarity between the original and the interpolated problem, the temporal position of interpolation, the degree of learning, the affective character of the material learned, and the conditions under which learning takes place. If the effectiveness of a given repetition upon learning is to some extent a function of the age of the associative connections, then, from the standpoint of retention, it is probable that the relative susceptibility of these associations to the detrimental effect of subsequent learning is to some extent also a function of the age variable. The question arises, then, as to which of two associations of the same strength but of different age is the more susceptible to retroactive inhibition. If, for example, maze habits are established in two instances so that they are of the same functional strength, as indicated by the level of efficiency in performance, but of unequal age, will the older or the younger maze solution be more detrimentally affected by the introduc-

* Accepted for publication December 12, 1932. The experimental data upon which this report is based were collected at Washington University, St. Louis, during 1931-1932.

[1] Adolf Jost, Die Assoziationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen, *Zsch. f. Psychol.*, 14, 1897, 436-472.

[2] H. C. Warren and L. Carmichael, *Elements of Human Psychology*, 1930, 211 ff.

tion of another maze problem before the retention test? The present experiment was conducted to answer this question.

Method of Procedure

It was necessary to establish for each of two groups of *Ss* certain associations of equal strength, the two groups differing from each other only in that the associations for one group had been established for a longer period than for the other. It was also necessary to introduce an interpolated problem during the interval before the retention test in one-half of the cases in each of the two 'age'-groups, in order that comparisons might be made as to the relative amount of retroactive inhibition. The amount of retroaction when the associations in the original problem have been established for a longer period can then be compared with the amount found when the associations have been more recently formed.

For the two learning problems two stylus mazes were used, which will be designated as Maze I and Maze II. Maze I in every case was used for the original problem, and Maze II was employed in every case for the interpolated problem. Maze I was a brass maze of the same pattern as that used by Carr in his experiment on visual guidance,[3] while Maze II was made of fiber-board and was not as difficult as Maze I. Diagrams of the maze patterns have been presented in another study.[4]

The *Ss* for the experiment were students from the Elementary Psychology classes of Washington University. There were 120 *Ss* (68 men and 52 women), none of whom had had previous maze experience.

The 120 *Ss* were divided into two groups of 60 each on the basis of the age of the associative connections, two ages being employed. They may be referred to as the older and younger 'age'-groups. In order to determine the relative amount of retroactive inhibition in the two cases, each age-group of 60 *Ss* was subdivided into two groups on the basis of the conditions of interpolation: one sub-group learned a second maze during the interval before the test for the retention of the original problem, and the other did not. Within each age-group, then, a comparison of the former sub-group with the latter on the basis of the efficiency of retention indicates the amount of retroactive inhibition.

The individuals were selected for a particular group in random order. The 4 groups were designated as: Group 1 (experimental), and Group 2 (control for Group 1); Group 3 (experimental), and Group 4 (control for Group 3). The following table, Table I, presents the program of the conditions of learning and relearning for each of the 4 groups.

The individuals in Group 1 learned Maze I to complete mastery. They relearned Maze I to the same criterion after an interval of 48 days, and were immediately given Maze II to learn. Immediately following the mastery of Maze II, they were given a retention test on Maze I.

Group 2 also learned Maze I to complete mastery, and after a 48-day interval relearned Maze I to the same criterion. However, after relearning Maze I, they were given a 20-min. rest period. Following this period they were given a retention test on Maze I.

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[3] H. A. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 399-417.

[4] S. H. Britt, The relationship between transfer of learning and age of previous associations, this JOURNAL, 46, 1934, 113-116.

Group 3 learned Maze I to mastery, then learned Maze II to mastery, and immediately took a retention test on Maze I.

Group 4 learned Maze I to mastery, rested 20 min., and then took a retention test on Maze I.

TABLE I

PROGRAM OF CONDITIONS OF LEARNING FOR EACH GROUP
(30 Ss in each group)

Group	Learning	Interval	Conditions employed to reach level of complete mastery	Conditions of interpolated interval	Retention test
1	Maze I	48 days	Maze I (Relearning)	Maze II	Maze I
2	Maze I	48 days	Maze I (Relearning)	20-min. rest*	Maze I
3			Maze I (Learning)	Maze II	Maze I
4			Maze I (Learning)	20-min. rest*	Maze I

* Refers to reading activity.

The criterion of complete mastery was the same in every case: 3 errorless trials in succession.

With Groups 2 and 4, the 20-min. rest period was chosen in order that the time-interval between complete mastery and the retention test would be comparable to the time-interval necessary for the work conditions of Groups 1 and 3. This 20-min. rest period consisted of reading a current issue of *College Humor*. *S* was instructed to begin reading wherever the first joke or cartoon appeared in the magazine and to note in writing those stories and cartoons which he considered funny or of humorous value. *S* was told that the scoring of the jokes was a second part of the experiment. This method was used for two reasons: first, it insured as much as possible against review of the solution of the problem during the rest interval; and, secondly, it filled the time-interval with fairly normal non-learning activities.

Three measures of learning efficiency were employed for each maze problem. The total number of trials, the total length of time in seconds (both of these records including the last 3 perfect trials), and the total number of errors were recorded.

In addition to the conditions just described, it was important to control two factors with respect to the associative connections in the original problem. (1) At the beginning of the 20-min. interval for which retention was to be tested, these associative connections in the two age-groups should be of unequal age. This factor was controlled by having Groups 1 and 2 learn the original problem 48 days before the time the problem was mastered by Groups 3 and 4. (2) At the beginning of this 20-min. interval, the associative connections in the two age-groups should be of equal functional strength. This comparability on the basis of functional strength was accomplished by having both age-groups meet the same criterion of mastery immediately before the 20-min. interpolated period. Thus, the individuals in the younger age-groups (Groups 3 and 4) mastered the original problem just prior to this interval; the individuals in the older age-groups (Groups 1 and 2) mastered the original problem 48 days before this interval and then, just prior to the 20-min. interval, relearned the maze to three errorless trials in succession. The purpose of this second mastery for the two older age-groups was to bring these older age-groups up to the same level of mastery as that which the younger age-groups had attained immediately

preceding the interval of retention. The associative connections at this point were of equal functional strength for all groups in the sense that all individuals in each group reached the same level of efficiency in terms of maze performance.

Groups 1 and 2 mastered Maze I two different times, before the 20-min. interval of retention, and hence had more practice on this problem than did Groups 3 and 4. It might be suggested that this element of additional practice would be an important factor in the final retention scores. This is probably true, but the criticism is not a serious one either of the procedure or of the results. In any situation in which one group of associations is older than another and the associations have been equated as to strength, it must necessarily be true that the older associations, simply because of the fact that they have been so equated, will have been more practiced than the younger or newer associations. It would have been possible in this experiment to give the older age-groups, Groups 1 and 2, the interpolated activity after the 48-day interval without the re-mastery of the original problem. However, in that case the functional strength of the associative connections in Groups 1 and 2, as the result of some forgetting during the interval, would not have been known and consequently it would have been impossible to equate the younger with the older age-groups on the basis of the strength of associations. Therefore it was considered to be better procedure to equate the older and the younger age-groups, just prior to the interval of retention, on the basis of the efficiency of maze performance by having all individuals meet the same criterion of mastery.

Results

The experimental results are presented under 4 separate headings: *(1)* the amount of retroactive inhibition occurring in the retention of the older associations; *(2)* the amount of retroactive inhibition found in the retention of the younger associations; *(3)* the relative amount of retroactive inhibition under conditions *(1)* and *(2)*; *(4)* the influence of the age of association upon retention.

(1) The amount of retroactive inhibition occurring in the retention of the older associations. The amount of retroaction that occurred in the retention of the older associations is obtained by comparing the retention scores of Group 1 with those of Group 2. These two groups are alike in that they learned Maze I and then relearned it 48 days later, just prior to the interval for which retention was tested. They differ as to the activities which filled this interval of retention. The interpolated activities were reading (Group 2), and learning another maze (Group 1). The influence of the interpolated maze upon the retention of the first maze-solution is indicated by the differences in retention scores between the two groups.

The relearning, recall, and saving scores of Groups 1 and 2 are shown in Table II. Differences between the two groups are presented in Table III. These data show that the acquisition of the second maze during the interval interfered with the retention of the first problem according to 6 of the 8 measures of retention employed. No measurable difference between the two groups appeared in terms of the amount of time taken in relearning the first maze. A slightly facilitating effect occurred, after the interpolation of the second problem, in terms of time of recall (amount of time taken on the first relearning trial). The detrimental effect of the interpolated problem

upon the retention of the previously learned maze solution is indicated by: an increase of 27.7% in the number of trials required in relearning, an increase of 45.3% in the number of errors made in relearning; an increase of 98.0% in errors of recall; and, in regard to saving scores, a 9.64% less saving in the number of trials, a 2.43% less saving in the amount of time, and a 1.27% less saving in the number of errors. Thus a comparison of the retention scores for the two groups indicates that the interpolated problem had a slightly detrimental effect upon the retention of the first problem in terms of saving scores, errors and trials of relearning, and errors of recall. The ratio of each difference to its σ (Table III) indicates that the differences are only fairly reliable. According to saving scores, the margin of superiority of Group 2 over Group 1 is not large, although the facts are consistent in indicating poorer retention under the interpolated learning condition.

(2) The amount of retroactive inhibition found in the retention of the younger associations. The second set of results are those obtained under the conditions in which the interpolated problem was given immediately after the complete mastery of the original problem. In order to determine the amount of retroaction in the younger associations, it is necessary to compare Group 3 with Group 4 on the basis of the efficiency of retention of Maze I. These two groups differed from each other only as to the activity during the interval between the mastery and the relearning of Maze I. The individuals in Group 4 rested for 20 min., while those in Group 3 learned Maze II.

The retention scores for each group are presented in Table II; comparative retention scores are shown in Table III. The influence of the interpolated maze upon the retention of the first maze was detrimental according to all criteria employed in measuring retention. The effect of learning the interpolated problem upon the retention of the previously learned maze solution was: in terms of relearning scores, an increase of 120.0% in the number of trials required, an increase of 230.3% in the amount of time taken, and an increase of 927.2% in the number of errors made; in terms of recall scores, an increase of 203.3%

TABLE II

RETENTION DATA FOR MAZE

Group	Relearning						% Saving			Recall				
	Trials		Time		Errors					Time		Errors		
	Av. no.	σ_{av}	Av. sec.	σ_{av}	Av. no.	σ_{av}	Trials	Time	Errors	Av. sec.	σ_{av}	Av. no.	σ_{av}
1	8.47	2.35	128.97	23.52	10.07	3.28	68.00	87.12	96.98	25.67	1.35	4.10	.77
2	6.63	.87	128.63	13.24	6.93	1.64	77.64	89.55	98.25	26.53	1.96	2.07	.65
3	15.03	1.68	428.60	1.35	117.83	59.01	28.21	57.78	63.59	74.63	18.64	34.90	11.07
4	6.83	.77	129.76	18.98	11.47	4.82	71.52	88.88	97.95	24.60	1.53	2.53	.85

in the amount of time taken, and an increase of 1279.4% in the number of errors made; and, in regard to saving scores, a 43.31% less saving in the number of trials, a 31.11% less saving in the amount of time, and a 34.36% less saving in the number of errors. Thus the facts are highly consistent in indicating a marked decrease in the efficiency of retention under the conditions of interpolated learning (Group 3) as compared with those conditions in which no second problem was mastered (Group 4). Statistically, the differences between the two groups in relearning and recall scores have a rather high degree of reliability, as indicated by the ratio of each difference to its σ (Table III). The chances in 100 that the direction of the difference would be confirmed by subsequent work are 96 or better.

TABLE III

GROUP DIFFERENCES IN RETENTION

Groups compared	Relearning			% Saving			Recall	
	Trials	Time	Errors	Trials	Time	Errors	Time	Errors
1 minus 2	1.84	.34	3.14	9.64	2.43	1.27	−.86*	2.03
σ diff.	2.51	26.99	3.67				2.38	1.00
D/σ diff.	.73	.01	.86				.36	2.03
%1>2	27.70	0.00	45.30				−3.20*	98.00
3 minus 4	8.20	298.84	106.36	43.31	31.11	34.36	50.03	32.37
σ diff.	1.85	136.33	59.21				18.70	11.10
D/σ diff.	4.43	2.19	1.80				2.68	2.92
%3>4	120.00	230.30	927.20				203.3	1279.4
3 minus 1	6.56	299.63	107.76	39.79	29.34	33.39	48.96	30.80
σ diff.	2.89	137.03	59.10				18.69	11.10
D/σ diff.	2.27	2.19	1.82				2.62	2.77
%3>1	77.40	232.30	1070.10				190.70	751.20
4 minus 2	.20	1.13	4.54	6.12	.67	.30	−1.93†	.46
σ diff.	1.16	23.14	5.09				2.49	1.07
D/σ diff.	.17	.05	.89				.78	.43
%4>2	3.00	.80	65.50				−7.20†	22.20

* Score of Group 2 greater than Group 1.
† Score of Group 2 greater than Group 4.

(3) The relative amount of retroactive inhibition in the retention of the older and the younger associations. In Sections *(1)* and *(2)*, immediately preceding, it has been shown that the interpolated problem, Maze II, had a detrimental effect upon the retention of the original problem, Maze I, in both the older and the younger age-groups. The next question concerns the relative amount of retroaction in the retention of the older and the younger associations. Are two groups of associations, which are of equal strength but of unequal age, equally susceptible to retroactive inhibition?

The relative susceptibility of the older and the younger associations to retroactive inhibition is obtained by a comparison of the differences between Groups 1 and 2 (showing retroaction for older associations) with those between Groups 3 and 4 (showing retroaction for younger associations). These data are shown

in Table III, and indicate the relative effect of the interpolated problem upon the retention of associations of different age. In terms of relearning trials, there was a detrimental effect of 27.7% for older associations, as compared with a detrimental effect of 120.0% for younger associations; on the basis of the time taken in relearning, no measurable retroaction was shown in the case of the older associations, while a detrimental effect of 230.3% occurred in the case of the younger associations; and, in terms of errors in relearning, a detrimental effect of 45.3% and 927.2% was shown for the older and the younger associations respectively. As to errors in recall, there was an increase of 98.0% for the older associations as compared with an increase of 1279.4% for the younger associations; and, as to the amount of time taken in recall, there was a facilitating effect of 3.2% for the older associations as compared with a detrimental effect of 203.3% for the younger associations. In other words, the older age-condition showed decidedly less susceptibility to the detrimental effect of the interpolated problem than did the younger age associations according to all relearning and recall scores. The same result is found when the two age-conditions are compared on the basis of saving scores. The effect of the interpolated problem upon the retention of the older associations was only slightly detrimental; the difference in the percentage saving, between Groups 1 and 2, was 9.64% in trials, 2.43% in time, and 1.27% in errors. The corresponding figures within the younger age-group, showing a decided decrease in the efficiency of retention, are 43.31% in trials, 31.11% in time, and 34.36% in errors. These comparisons indicate clearly that the amount of retroactive inhibition resulting from an interpolated maze problem varies with the age of the associations of the original problem. A decidedly greater amount of retroactive inhibition occurred in the retention of the younger than in the retention of the older associations.

A second means of determining the relative amount of retroactive inhibition in the two age-groups is to compare the raw scores on the final retention test of Group 1 with those of Group 3. These two groups had the same maze problem interpolated immediately preceding the retention test; they differed from each other in that the associative connections for the original problem were established for a much longer period of time (48 days) in Group 1 than in Group 3. Comparative retention data are presented in Table III. The differences between Groups 1 and 3 are pronounced, according to all criteria of retention employed; and the various measurements are consistent in indicating greater efficiency of retention of the older than the younger associations.

(4) The relation of the age of the associations to the efficiency of retention. A comparison of the retention records of Groups 2 and 4 indicates the relation of the age of associations to the efficiency of the retention of Maze I, when no second problem is interpolated during the 20-min. interval. The amount retained under these two conditions is shown in Table II. Comparative retention data are presented in Table III. These data indicate that older associations probably "fade less rapidly" than younger. According to 7 of the 8 criteria employed in measuring retention, the maze solution is more effectively retained in the older age-group than in the younger. Although the facts are rather consistent in indicating this result, the margin of superiority of Group 2, the older age-group, over Group 4 is small, and, statistically, the differences are of low reliability.

Discussion of Results

If two associations are of the same functional strength but of unequal age, the older association is apparently less susceptible to retroactive inhibition than the younger association. However, this does not mean that the older of two associations of equal strength is more resistant, by virtue of its age, to the detrimental effects of activities learned subsequently. To state that the age of the original habit is greater in one group than in the other, at the time of the interpolated acquisition, is very probably nothing more than a convenient way of calling attention to the fact that the retentive process underlying the habit has been subjected to such things as greater amount of activity and different sequence of events. Whenever the age-factor in such a situation is varied, as in the present instance, other factors are also necessarily varied, such as distribution of effort, amount of practice, and familiarity with the test situation. These factors are probably of great importance in accounting for the relationship that has been found between the age of the associations and susceptibility to retroaction.

In the present experiment, the older age-group learned the original problem at one sitting, and 48 days later practiced the same problem until a definite level of efficiency in performance was reached. Their effort was thus distributed into two different work periods before the introduction of the second problem. On the other hand, the younger age-group learned the original problem at one sitting and was given the second problem to master immediately thereafter. The greater distribution of effort in the older age-group may be definitely related to the decrease in susceptibility to retroaction under this condition, as contrasted with the susceptibility to retroaction in the younger age-group.

A second variable, probably of some importance in this connection, is the amount of practice. The introduction of a second practice period in the older age-condition resulted not only in a greater distribution of the total effort on that problem, but also in a greater amount of practice in the maze for which retention was later to be tested.

Another variable deserving to be mentioned is the amount of the interpolated activity. The amount of interpolated activity is probably an important factor in this connection, for it refers to a very significant characteristic of the activity designed to produce retroaction. Maze II was used as the interpolated problem for both age-groups. However, the younger age-group had a great deal more interpolated activity than the older age-group, as indicated by the following scores in mastering the second maze: the older age-group took 21.63 trials, 632.83 sec., and made 203.63 errors, whereas the younger age-group took 39.20 trials, 1195.10 sec., and made

478.46 errors. Hence, Group 3, the younger age-group, took almost twice as long, required approximately twice as many trials, and made twice as many errors as did Group 1 in learning the interpolated problem. Although the interpolated problem was the same, the amount of interpolated maze activity was not. The possibility that the amount of retroactive inhibition may vary with the amount of interpolated activity has been noted,[5] although this factor has not been the subject of any experimental investigation.

As indicated above, the interpolated maze was mastered in fewer trials, with fewer errors, and in less time, by the older than by the younger age-group. There was greater positive transfer from the older than from the younger associations to the learning of the second maze;[6] and the interpolated problem interfered less in the retention of the older than in the retention of the younger associations. This indication is in agreement with results obtained by Webb from which he concluded that "there is a negative correlation between positive transfer and negative retroaction. Those conditions which produce the maximum amount of positive transfer give the least amount of negative retroaction."[7] Some suggestions of this relationship noted by Webb appeared also in another study in which the interpolated maze was learned more quickly but had less detrimental effect upon the retention of the original maze when the latter was mastered under punishment conditions, as contrasted with normal non-punishment conditions.[8]

Summary and Conclusions

The present investigation was conducted to determine the relationship between susceptibility to retroactive inhibition and age of associations when the associations are of equal strength, two age-conditions being employed. In the younger age-condition the original maze, for which retention was to be tested later, was mastered immediately preceding the interval (20 min.) of retention. In the older age-condition, the *S* mastered the original maze and then 48 days later, just prior to the 20-min. interval, mastered the original problem again and thus reached the same level of efficiency in maze performance as that exhibited at this time by the younger age-condition. One-half of the *S*s in each age-group learned a second maze during the 20-min. interval, and the other half did

[5] M. E. Bunch and F. D. McTeer, The influence of punishment during learning upon retroactive inhibition, *J. Exper. Psychol.*, 15, 1932, 473-495.

[6] The relationship between age of associations and transfer is discussed by Britt, *op. cit.*, 115 f.

[7] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.*, 24, 1917, (no. 104), 83.

[8] Bunch and McTeer, *op. cit.*, 494.

not. The amount of retroactive inhibition when the associations in the original problem had been established for a longer time was compared with the amount found when the associations had been more recently formed. The results justify the following conclusions:

(1) Of two associations (maze habits) of equal strength but of unequal age, the younger is more susceptible to retroactive inhibition. The data are highly consistent in indicating this relationship.

(2) The second part of Jost's Law, that the older of two associative connections fades less rapidly than the newer, is substantiated in general.

(3) The older age-group exhibited less retroaction in the retention of the original problem and greater efficiency in the mastery of the interpolated problem than did the younger age-group. This suggests that when age of association is the variable, the relationship between the amount of positive transfer and the amount of retroaction is an inverse one.

NEW PHENOMENA OF APPARENT VISUAL MOVEMENT

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The large amount of work on the problems of apparent movement since the work of Wertheimer[1] has been adequately summarized by Mibai[2] so that further reference seems unnecessary in this place. In practically all of these experiments, however, single objects were employed as stimuli. One notable exception is the experiment of Wertheimer with falling lines to demonstrate that apparent movement could take place simultaneously in two directions. The present experiment differs from the previous ones by employing double stimuli.

APPARATUS AND METHOD

Stimulus-cards were exposed in a modification of the Dodge tachistoscope so arranged that two illuminated stimulus-cards would appear successively in the same field. Combined with the tachistoscope was a modification of the timing device described by Jenkins[3] which utilizes a condenser-charge apparatus so that either one or the other chamber, at the end of which is placed a stimulus-card, is successively illuminated. The two chambers are at right angles to one another and the images of the two test cards are brought into the same field for the *O* by inserting a half silvered mirror at an angle of 45° in the lines between the eye and the two test cards. The timing device was arranged so that the interval of exposure was 124 sigma. In other words, Test Card 1 was in the field at the beginning of each experimental situation. After a 'Ready,' 'Now' signal, the key was pressed which changed the illumination from Test Card 1 to Test Card 2 for 124 sigma and then, automatically there was a change of illumination from Test Card 2 back to Test Card 1.

Fifty members of the department staff and graduate students in psychology at the University of Pennsylvania acted as *Os*. Each experimental situation was repeated several times until the *O* was prepared to give a complete introspective description of what had occurred. Instructions were given the *Os* as follows.

You will see two (or one) figures in the visual field. After I give you a 'Ready,' 'Now' signal, there may be some change in these figures in the field. I am primarily interested in having you report any changes in spatial relations. Each stimulus situation will be repeated as often as you wish until you are ready to describe the changes, if any should occur.

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[1] M. Wertheimer. Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

[2] S. Mibai. An experimental study of apparent movement, *Psychol. Mon.*, 42, 1931, (no. 190), 1-91.

[3] For a description of the apparatus cf. J. G. Jenkins, A simple tachistoscope of many uses, this JOURNAL, 45, 1933, 150; and W. D. Moeder and J. G. Jenkins, A condenser-charge timer, *ibid.*, 45, 1933, 742-744. Although certain changes were made in the form of the apparatus of the present experiment, the general principles are the same as those found in these two papers.

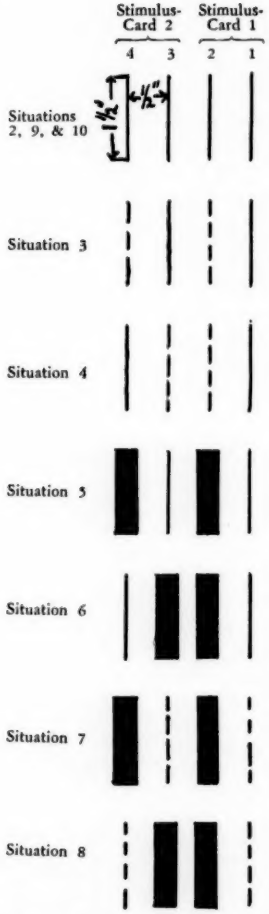

FIG. 1. THE STIMULUS-CARDS
Lines 1 and 2 are on first card and 3 and 4 on the second card. Broken lines represent red. In exposure field, line 2 on Card 1 and line 3 on Card 2 were in identical spatial positions.

It was seldom that *O* required more than four or at most five exposures of any stimulus-situation. No attempt was made to control fixation except in Situations 9 and 10. The stimulus-situations are indicated in Fig. 1. In this figure the spatial relations as they appeared in the visual field are maintained as far as possible. Lines 1 and 2 were on Card 1 and lines 3 and 4 were on Card 2. The two central lines (2 on Card 1 and 3 on Card 2) were in identical spatial position in the exposure field. Situation 1 (not given in Fig. 1) consisted of a single line on each card in the positions of line 2 on Card 1 and line 4 on Card 2. This experience was given so that all *O*s might have the experience of apparent movement of a simple form under laboratory conditions before the beginning of the experiment proper. Situations 9 and 10 utilized the same stimulus situations as was already employed in Situation 2. In Situation 9, *O* was asked, before the signal, to fixate line 1 and in Situation 10, he was asked to fixate line 2.

RESULTS

Situation 1. (Single line on each card.) Every *O* reported apparent movement of the single line to the left (*O*'s left) and back to original position. The extent of this movement was variously estimated as being from ½ in. (the actual distance between the two lines on the field) up to as much as 2 in. The average estimate of the extent of movement was 1.11 in.

Situation 2. (Two similar black lines on each card.) In the case of every *O* these lines were reported as apparently moving in parallel fashion to the left and then apparently moving back to the original position again in parallel fashion. In the present case, the extent of movement was estimated at less than in Situation 1. The estimated range ran from ¼ to 1½ in. and the average for this second situation was only 0.78. Only one *O* estimated that the apparent movement in Situation 2 was greater than in Situation 1; 19 *O*s estimated that the two apparent movements were the same and the other 30 *O*s reported that the apparent movement in Situation 2 was less than that in Situation 1.

Situation 3. (Narrow red and black line on each card; red line to left in both cases.) Invariably apparently parallel movement was reported first to the left and then back to the right to the original position. For 41 *O*s this apparent movement was reported as entirely parallel for both lines. For the other 9 *O*s, the apparent movement was relatively parallel but for these *O*s the red line seemed to move slightly more to the left than the black line.

Situation 4. (Narrow red and black line on each card; on Card 1 black line to right of red and on Card 2 black line to left of red line.) In this situation 42 *O*s report that the red line remains either stationary or relatively stationary and that the black line moves first to the left, apparently jumping the red line and then back to its original position. Five *O*s report an interchange of the lines, each line apparently moving in the opposite direction, crossing one another and then crossing back. Three *O*s report that both lines apparently moved in parallel fashion to the left and changed color and then apparently moved back to the right with another change of color.

An interesting phenomenon was observed in this situation for the first time. In practically all cases in which the red line was apparently stationary or practically stationary and was 'jumped' by the black line, this crossing occurred in the third dimension. For only 6 *O*s, the apparent crossing occurred in the plane of the card; for 24 *O*s the apparent crossing in both directions occurred by the apparently moving line coming out to the forward plane in front of the cord; for 7 *O*s this occurred in the backward plane. Four more *O*s also reported a circular apparent movement—the apparently moving line crossing in one direction in front of the other and, in the other direction, behind the apparently stationary figure.

There is variability of report, in this situation, as to what happens to the central (red) line. Sixteen *O*s reported that this line remained absolutely stationary, while 24 *O*s reported that it seemed to move slightly to the left and back again.

Situation 5. (Narrow and broad black line on each card with broad line always to the left.) For every one of the 50 *O*s, apparent parallel movement was reported—at first to the left and then to the right. Seven *O*s report that this movement was not exactly parallel and that either the thick or the thin line apparently moved more than the other.

Situation 6. (Narrow and broad black lines on each card; on Card 1 broad line to left and on Card 2 broad line to right.) In this case 47 *O*s report that the heavy line remained exactly or approximately stationary and that the thin line apparently crossed the heavy line to assume a position to its left and then crossed back again. The other 3 *O*s report an interchange of positions with both lines apparently moving in opposite directions. In this case 24 *O*s report that the apparent 'jump' is in the plane in front of the card in both directions; 12 report the apparent 'jump' in the plane in back of the card; 4 in the plane of the card; and 3 report circular movement. For this situation 21 *O*s report that the thick (center) line remained absolutely stationary and 21 *O*s report that it apparently moved slightly to the left.

Situation 7. (Broad black and narrow red line on each card; broad black line always to left.) In this situation all 50 *O*s unanimously reported apparent parallel movement to the left and back to the right. In only 1 case was a difference in the extent of the movement reported.

Situation 8. (Broad black and narrow red line on each card; on Card 1 narrow red line to right and on Card 2 narrow red line to left.) In this situation, all 50 *O*s unanimously reported that the broad black line remained actually or practically stationary and that the narrow red line apparently crosses it to the left and back again to the right. The following distribution of the planes of apparent movement of the red line were reported: in front plane both ways, 28; in back plane both ways, 12; in plane of card both ways, 4; circular movement, 3. With regard to the broad (center) line, 30 *O*s reported that it remained absolutely stationary and 20 *O*s reported that it also apparently moved very slightly to the left.

Situation 9. (Two narrow black lines on each card. In the first card *O* was instructed to fixate the line to his right.) In this situation, 9 *O*s report that the right hand line apparently jumped across the left hand line and then jumped back again to its original position while the left hand line on Card 1 remained actually or relatively stationary. The other 41 *O*s reported that the two lines apparently moved in parallel fashion first to the left and back again to the right.

Situation 10. (Two narrow black lines on each card. In the first card *O* was instructed to fixate the line to his left.) In this situation 17 *O*s reported that the fixated (left) line remained stationary while the other line apparently 'jumped' across it to the left and back again. The other 33 *O*s reported apparently parallel movements of the two lines.

Discussion

Many effects already noted in the literature were also reported by our *O*s, such as, for example, lines receding into the third dimension, different apparent rates of movement of parts of the same line, the central line apparently remaining stationary but turning on its axis and the like. It seems hardly worth while to record these secondary phenomena in this place. Our primary problem was to determine if we could set up physical situations which would be the basis for different phenomenal reports of apparent visual movement. The results are perfectly plain in this point. Situations 2, 3, 5, and 7 presented physical arrangements which would have been accomplished by parallel movement if actual movement had been present and, in every case, apparent parallel movement was reported. On the other hand, Situations 4, 6, and 8 presented physical arrangements which would have been accomplished by one figure remaining stationary and the other figure crossing it, if actual movement had been present, and in almost every case in Situations 4 and 6 and in every case in Situation 8 this apparent phenomenal crossing was reported. In the other reports, the phenomenal description was of a possible apparent movement to accomplish this physical result although, perhaps, not the most obvious one.

Our three criteria of differentiation of the two figures in Situations 4, 6, and 8 were respectively color only, size and shape only, and color and size and shape. As one goes up this scale of differentiation the reports of apparent phenomenal crossing increase as follows: color only, 42; form only, 47; form and color, 50. It would seem that in these findings we have a most excellent demonstration of the "constancy hypothesis" of *Gestaltheorie* in respect to the relation between physical and phenomenal pattern.[4] The fact that with greater and greater differentiation

[4] W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Zsch. f. Psychol.*, 66, 1913, 51-80; Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresber. ü. d. ges. Physiol.*, 1922, 516. H. Nelson, The psychology of *Gestalt*, this

of the two figures in Situations 4, 6, and 8, the center figure remains more often stationary also lends support to this conclusion. The times the center figure was reported stationary were as follows: color only, 16; form only, 21; color and form, 30. In other words, greater differentiation of the two figures tends more and more to 'anchor' the central figure.

Still another confirmation of the "constancy hypothesis" is to be found in the frequency with which this apparent crossing in Situations 4, 6, and 8 involved apparent movement in the third dimension. Third dimensional apparent movements were reported as follows: color alone, 35; form alone, 39; and color and form, 43. It is also interesting to note in this connection that 27 *O*s report apparent movement in the same tridimensional plane for all three situations. Inasmuch as this is a purely subjective contribution, one might expect that an individual would apparently see all such movement either in the forward or backward plane or the plane of the card in some such consistent fashion.

Having established this relatively constant relation between the physical situation and the phenomenal report, it seemed worth while, in Situation 9 and 10, to determine what effects certain subjective contributions might make toward the differentiation of the two kinds of apparent movement in a situation which might be considered physically ambiguous. For this purpose, we employed the exactly similar pairs of lines for which, as Situation 2 but without prescribed fixation, the *O*s invariably reported apparent parallel movement. When the right hand line (outside line) was fixated, 9 *O*s only reported the 'jump' type of apparent movement. When the left hand (central) line was fixated the number of 'jump' movements increased to 17. It would seem that fixation of the center line has a slight tendency to 'anchor' this line but the effect is not nearly as great as the differentiation in the types of apparent movement produced by the physical situation.

One last point of interest is the difference in the extent of movement in Situations 1 and 2. In Situation 1, with the apparent movement of a single line, the average estimated extent was 1.11 inches as against 0.78 inches for Situation 2 with the pairs of lines. In this latter situation, the second line apparently gave a point of reference in the field for the movement, which was obviously lacking in the apparent movement of the single line. The situation, although not similar, conforms in general to the observation of Rubin of the differences of the movement of a single light on the edge of a large spool when revolved alone or when revolved when a second light is placed in the center.

Conclusions

(1) By employing two figures on each of two test cards tachistoscopically exposed so that they appeared on the same field, it was possible to arrange conditions to produce two kinds of apparent visual movement.

(2) If the physical conditions favored apparently parallel movement of the two figures, this type of movement was always reported.

(3) If the physical conditions favored one figure remaining stationary and the other figure crossing it, this type of movement was almost always reported.

(4) Apparent movement of this latter type almost always proceeded through a plane either in front of or in back of the stationary figure.

Journal, 36, 1925, 350-353. H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, *ibid.*, 37, 1926, 469-501.

(5) The greater the differentiation between the two figures, the more frequently is this second type of apparent movement reported.

(6) These findings give strong confirmation of the "constancy hypothesis" of *Gestaltheorie* in respect to the relation between physical and phenomenal pattern.

(7) Fixation of one of the two figures is less effective in establishing one or another of the two types of apparent movement than is the physical pattern.

INCIDENTAL MEMORY FOR PLEASANT, UNPLEASANT, AND INDIFFERENT WORDS

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Although a number of investigations have been made of the learning and retention of pleasant and unpleasant activities, relatively few of these studies have been concerned with incidental memory. Previous studies of the learning and retention of mildly pleasant, mildly unpleasant, and indifferent words have been carried out by Tait, Griffitts, Anderson and Bolton, Chaney and Lauer, Thomson, and Cason.[1] In view of the general importance of the subject, it has seemed desirable to carry out a further experiment using a different procedure and a larger number of subjects. In the present paper we are principally concerned with describing the results of this further investigation.[2]

Material. A total of 468 different words were used, which were the same as those used in a previous study. The principal differences between the words were in the pleasant, unpleasant, and indifferent feelings that were attached to them.[3] Each of the 468 words was printed on a ½ x 1½ in. stiff card, and the cards were thoroughly mixed. Each of the words was used once in making up 4 lists of 117 words each; these lists will be referred to hereafter as the *original lists*. They were made approximately equal to each other in the number of nouns, verbs, and adjectives which were included. Each of the original lists of words was typewritten double-spaced in 4 columns on an 8½ x 11 in. sheet of paper. An *alphabetical list* of all of the 468 words was also prepared, typewritten single-spaced on two sheets of paper. The alphabetical list was used with each *S*, but only one of the 4 original lists was used with a single *S*. The 4 original lists were rotated with different *S*s so that each original list was used approximately the same number of times.

Procedure. Silverman was *E* for all of the *S*s. The procedure was individual, and the experimental period generally lasted for one hour. The following procedure was carefully followed with each *S*.

(1) After being seated at a table *S* was given one of the original lists of 117

* Accepted for publication July 1, 1932.

[1] W. D. Tait, The effect of psychophysical attitudes on memory, *J. Abnorm. Psychol.*, 8, 1913, 10-37; C. H. Griffitts, Results of some experiments on affection, distribution of associations, and recall, *J. Exper. Psychol.*, 3, 1920, 447-464; A. C. Anderson and F. J. Bolton, Inhibition of the unpleasant, *J. Abnorm. & Soc. Psychol.*, 20, 1925, 300-302; R. M. Chaney and A. R. Lauer, The influence of affective tone on learning and retention, *J. Educ. Psychol.*, 20, 1929, 287-290; R. H. Thomson, An experimental study of memory as influenced by feeling tone, *J. Exper. Psychol.*, 13, 1930, 462-468; Hulsey Cason, The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134), 1-96.

[2] A bibliography on the relation between feeling and memory may be found in Cason, *op. cit.*, and references to the studies on incidental memory are given in S. M. Shellow, Individual differences in incidental memory, *Arch. Psychol.*, 10, 1923, (no. 64), 1-53.

[3] See Cason, *op. cit.*, 39-41, 44-46.

words, and was asked to grade each word P, I, or U[4] according to his personal reactions; that is, on the basis of the immediate associations evoked by the word or on the basis of his feelings towards the word itself. He was instructed to proceed slowly enough to give each word a fair chance, but not so slowly that his grades would not be representative of his ordinary affective reactions to the words. He was requested to spend the same amount of time in grading each word. It was suggested that he give approximately the same number of grades of P, I, and U, but he was urged to grade the words in such a way that his grades would be typical and representative of his own personal reactions. He was assured that grading the words P, I, and U had no relation to intelligence or to any other personality traits.

Twelve of the 73 *Ss* spoke the grades aloud as they wrote them so that *E* could obtain a measure of the relative time required to decide upon the grades of P, I, and U. The number of seconds spent in giving grades of P, I, or U for 1356 words was measured, the first word in each of the 4 columns of the original list not being used in this procedure.

(2) *S* was required to go through the original list of 117 words a second time and to examine all of his grades again in order to be more certain that the grades which he had assigned were typical and representative of him. He was told that an average of about 6 grades had been changed by the previous *Ss*, but that he could change as many or as few as he wished. A record was obtained of all of the changes that were made in the grades. The revised or final grades were used in calculating the results. Although it is unlikely that strong feelings were present while the words were being graded P, I, and U, the majority of the words were associated with incidents and events which had or had had a definite emotional coloring for *S*. Most of the *Ss* enjoyed grading the words.

(3) During a rest period of 5 min., *E* attempted to prevent *S* from thinking about the words and the grades which he had assigned by engaging him in conversation about daily events and ordinary school matters. If *S* talked about the words he had graded *E* tried to change the topic of conversation.

(4) *S* was given a blank sheet of paper and instructed to write as many of the words in the original list as he could recall. He was asked to try to recall and write the words themselves, but not to write the grades of P, I, and U which he had given them. If he inquired about spelling he was told that only correctly spelled words would count. *S* was encouraged to continue trying to recall the words during the whole of the 5-min. period that was allowed for this part of the procedure.

(5) There was a second rest period of 5 min. as in (3) above.

(6) *S* was given the alphabetical list of 468 words, which included the 117 words in his original list, and was instructed to underline all words that had been present in the original list. The general difference between positive and negative recognition was explained to him. If he asked about guessing, he was told that this was permissible, but that he should not underline more than ¼ of the words in the alphabetical list.

(7) At the end of the experimental period, *S* was asked a number of questions about the general methods that he had used and about his insight into the purpose and nature of the experiment.

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[4] P = pleasant; I = indifferent; and U = unpleasant.

Subjects. Thirty-seven of the 73 *Ss* used were men and 36 were women. In the group there were 8 freshmen, 18 sophomores, 23 juniors, 21 seniors, and 3 graduate students.

Results. A count was first made of the number of words in the original list that were graded P, I, and U by each *S*. The average number of words graded P by the 73 *Ss* was 43.4, or 37.1% of the total number of words (117); the average number graded I was 37.8, or 32.3%; and the average number graded U was 35.8, or 30.6%. The median numbers of words were practically the same, being 43 for P, 37 for I, and 36 for U. Considering the method originally used in selecting the 468 words that were used in the present study, the *Ss* should theoretically have given the same number of grades of P, I, and U for these words.[5]

The measure that was obtained of the time used by 12 of the *Ss* in deciding upon the grades of P, I, and U was the number of seconds which elapsed between the speaking of the preceding grade and the speaking of the present grade. The average number of seconds which elapsed while the grades were being decided upon was, for P grades, 3.6 ($n = 476$); for I grades, 4.1 ($n = 476$); and for U grades, 3.7 ($n = 404$).

It will be recalled that after grading all of the words in the original list each *S* reëxamined these grades and changed any that he desired. The total number of changes in grade for individual *Ss* ranged from 1 to 17, with the average at 5.6 and the median at 5.0. All the changes in grade that were made may be represented as follows:

First grading	Second grading			
	P	I	U	Total
P	—	58	29	87
I	122	—	109	231
U	31	60	—	91
Total	153	118	138	409

These figures indicate that 122 grades were changed from I to P, etc. The principal tendency was to change the I grades to P or U.

S graded each of the 117 words in his original list P, I, or U, and the percentage of each of the three kinds of words that were recalled in Procedure 4 was calculated for every *S*. If *S* graded 40 of the 117 words in the original list P and recalled 8 of these P words in Procedure 4, then his percentage of P words recalled was 20. Using these scores for individual *Ss* as the basis of calculation, the average and median percentages recalled for the 73 *Ss* were as follows.

	Percentage recalled	
	Av.	Median
P	21.0	20.8
I	14.2	14.6
U	17.9	17.0

The relative order of rank in the efficiency of recall for the 3 different grades or for the 3 different kinds of words was P-U-I.

[5] Cf. Cason, *op. cit.*, and Association in relation to feeling and gross bodily movement, this JOURNAL, 46, 1934, 213.

A calculation was likewise made for each *S* of the percentage of each of the three kinds of words that were recognized in Procedure 6. If *S* graded 40 of the 117 words in the original list P and recognized 30 of these P words in Procedure 6, then his percentage of P words recognized was 75. Using these scores for individual *S*s as the basis of calculation, the average and median percentages recognized for the 73 *S*s were as follows.

	Percentage recognized	
	Av.	Median
P	71.2	75.4
I	62.6	64.0
U	69.4	70.4

The relative order of rank in the efficiency of recognition for the 3 different grades or for the 3 different kinds of words was also P-U-I, but some of these differences are not very large.

In the procedure that was followed, it will be recalled, *S* first gave a grade of P, I, or U for each of the words in the original list; then he was unexpectedly asked to recall as many of these words as possible, and finally he was given the alphabetical list of 468 words, which included the original list of 117 words, and a measure was obtained of his ability to recognize the words in the original list. Inasmuch as writing and thinking about certain words in the recall procedure had some influence on the ability to recognize the same words later, one further calculation of the results has seemed desirable. In the case of each *S*, a calculation was made of the number of words originally graded P, I, and U which were not recalled in Procedure 4. Then the percentage of these words which were not recalled in Procedure 4 but which were recognized in Procedure 6 was calculated for each *S*. If, for example, 40 of the words in the original list were graded P and 8 of these were recalled in Procedure 4, then 32 words were of course not recalled. If among these 32 words not recalled in Procedure 4, 24 were recognized in Procedure 6, then the percentage of words not recalled but later recognized was 75. Using these individual scores from each of the *S*s as the basis of calculation, the average and median percentages of words not recalled but later recognized were as follows for the 73 *S*s.

	Percentage not recalled but later recognized	
	Av.	Median
P	66.1	68.7
I	57.8	59.4
U	64.5	66.6

The same relative order of rank in the efficiency of recognition for the 3 different grades or for the 3 different kinds of words was obtained as before, namely P-U-I.

The retrospective reports of the *S*s at the end of the experimental period are of some importance in the general interpretation of the results. The majority of the *S*s assigned grades of P, I, and U for the words in the original list on the basis of associations with recent events; several *S*s graded the words on the basis of associations with early events, and a few gave their grades on the basis of associa-

tions with people. Most of the changes in grade were made because the *S* recalled some event in the second grading which he had not recalled in the first grading. None of the *Ss* expected the test for recall in Procedure 4. Most of them said that while they were trying to recall the words in Procedure 4 a few words would appear, and then several others would be recalled mainly by association with the words that first came to mind. In only a few cases was recall facilitated by visualizing the original list of words, and thinking about the grades which had been previously assigned was a relatively unimportant factor. The test for recognition was unexpected for all of the *Ss* except one. The majority of the *Ss* felt more certain about positive than about negative recognition. During and at the end of Procedures 1 and 2, most of the *Ss* thought that grading the words P, I, and U was the purpose of the experiment. At the end of the experimental period, most of them thought that the experiment was intended to measure their memory for the words.

Although the order of rank for efficiency in the tests for recall, for recognition, and for words not recalled but later recognized was P-U-I, the differences between P and U and between U and I were relatively small. The results seem to support the conclusion, as previously stated by Cason, that "there is little, if any, difference in the efficiency with which P and U activities can be learned and reproduced."[6]

Although an attempt was made to have the *Ss* give the same amount of attention to the P, I, and U words while they were grading them in Procedures 1 and 2, we are not certain that the experimental conditions can be entirely controlled in this respect. In several of the studies on the learning and retention of pleasant and unpleasant activities, the factors of desire, interest, effort, and later learning seem to have produced a relative increase in the efficiency of retaining and reproducing activities that were distinctly P when they originally occurred as compared with activities that were distinctly U when they originally occurred. In the present experiment, however, these factors were controlled to such an extent that the obtained rank order of P-U-I seems to require some other explanation.

The results do not suggest that there was a marked tendency for the P words to be 'stamped in' or for the U words to be 'stamped out.' There was in fact a clearer tendency for both the P and the U words to be recalled and recognized more efficiently than the I words. During the short interval over which retention was measured there was no observable tendency towards the obliviscence of the disagreeable.

Summary and conclusions. Seventy-three *Ss* graded their feelings towards 117 words as pleasant, indifferent, or unpleasant principally on the basis of their personal associations with the words. A few minutes later they were unexpectedly asked to recall as many of these words as possible. The last part of the procedure consisted in obtaining a measure of the *Ss*' recognition memory for the same list of 117 words. During the 5-min. rest periods, which were given between the original grading of the words and the test for recall and between the recall test and the recognition test, *E* attempted to distract *S* by conversation about ordinary events. The principal conclusions which seem justified from the experiment are as follows.

(1) A larger number of the words in the original list were graded P than were graded I, and a larger number were graded I than were graded U.

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[6] *Op. cit., Arch. Psychol.,* 21, 1932, (no. 134), 75-80.

(2) Less time was taken in deciding upon the grades of P or U than in deciding upon the grades of I.

(3) When *S* examined his grades of P, I, and U the second time, and was asked to change any that he thought should be changed, the majority of the changes in grade that were made were from I to P and from I to U.

(4) When the calculations were based on all of the percentage scores of the individual *S*s, the relative order of efficiency for the 3 different grades was P-U-I for recall, for recognition, and also for words not recalled but later recognized. It should be noted, however, that this consistent rank order for P, U, and I is based on the averages and medians of the combined results of all of the *S*s, and that the relative ranks of P, I, and U for individual *S*s are quite irregular.

THE DIAGNOSTIC VALUE OF INTROVERSION-EXTRAVERSION ITEMS

By Ross Stagner and Joseph Pessin, University of Wisconsin

One of the major problems in personality test construction is to select valid and reliable items. If, for example, one is to construct a test to determine introversion-extraversion, one must select test items which on repetition will yield the same results, which test the alleged traits, and which differentiate adequately those possessing introvert tendencies from those having extravert tendencies. The first personality tests contained items which were put there by *a priori* assumptions of the test makers, or by the opinions of one or more 'expert' judges. Recently attempts have been made to determine empirically the value of test items selected by rational methods or opinions. Among these attempts may be mentioned the studies of Garrett and Schneck[1] and Heidbreder.[2]

The purpose of the present study is to determine objectively the diagnostic value of introvert-extravert items used in current personality tests. We may state our purpose in the form of a question; how well do test items differentiate between extraverts and introverts?

Procedure. One hundred and forty items were selected from the following 5 widely used personality tests: Marston,[3] Laird C 2,[4] Freyd and Heidbreder,[5] Conklin,[6] Neymann and Kohlstedt.[7] Since there were a large number of items available, it was decided to select only the clearly non-duplicate items. The 140 items were combined in the form of a questionary consisting of two parts the first half referred to likes or dislikes,[8] that is, to *personal preferences;* the second half referred to *personal habits.*

The questionary thus constructed was administered to 171 men in the elementary psychology class during the first semester 1931-32. Men only were used, to avoid the problem of sex differences which must necessarily be considered in such a study (although Heidbreder claims that items diagnostic for men are diagnostic also for women).[9] The tests were scored by an *a priori* key made up from

* Accepted for publication June 20, 1932.

[1] H. E. Garrett and M. R. Schneck, A study of the discriminative value of the Woodworth Personal Data Sheet, *J. Gen. Psychol.,* 1, 1928, 459-471.

[2] E. F. Heidbreder. Measuring extroversion and introversion, *J. Abn. & Soc. Psychol.,* 21, 1926, 120-134.

[3] L. R. Marston, Emotions of young children, *Univ. Iowa Stud. Child Welfare,* 3, 1925, 22-23.

[4] D. A. Laird, Detecting abnormal behavior, *J. Abn. & Soc. Psychol.,* 20, 1925, 138-141.

[5] M. Freyd, Introverts and extroverts, *Psychol. Rev.,* 31, 1924, 74-87. The list proposed by Freyd was made into a test by Heidbreder (*op. cit.*).

[6] E. S. Conklin, Determination of normal extravert-introvert interest differences, *Ped. Sem.,* 34, 1927, 28-37.

[7] C. A. Neymann and K. D. Kohlstedt, New diagnostic test for introversion-extroversion, *J. Abn. & Soc. Psychol.,* 23, 1929, 482-87.

[8] To avoid overweighting the limited range of likes and dislikes tested by Conklin (*op. cit.*), only 15 of his items were employed.

[9] Heidbreder, Introversion and extroversion in men and women, *J. Abn. & Soc. Psychol.,* 22, 1927, 51-61.

the different tests using these items, giving the introverted answer a weight of 1 in each case. Thus in the final computation a high total score was indicative of introversion and a low score of extraversion.

To determine the diagnostic value of each item, the extremes of the distribution were selected as criterion groups. Approximately 20% of the total number of cases was in each criterion group. On the personal preference part of the questionary the extravert group included 32 *Ss*, and the introvert 31. For the second half of the questionary, the personal habits section, 28 individuals were selected as extraverts and 32 as introverts.

TABLE I

ITEMS IN THE PERSONAL HABITS GROUP HAVING GREATEST DIAGNOSTIC VALUE (Arranged in order)

	Critical ratio values	Introvert answer
Are you habitually absentminded?	12.33	yes
Are your feelings hurt by remarks or actions referring to you?	8.09	yes
Do you often worry about possible misfortunes?	7.02	yes
Are you often self-conscious in front of strangers?	5.82	yes
Are you introspective (turn your attention inward to your own thoughts and ideas)?	5.32	yes
Do you often hesitate on making decisions in the ordinary course of the day?	4.85	yes
Are you often considerably upset when unsuccessful in any undertaking?	4.64	yes
Do you usually enjoy the story rather than the way it is written?	4.42	no
Do you usually pay serious attention to rumors?	4.41	yes
Have you often avoided members of the opposite sex?	4.30	yes
Do you often brood over failure in a game or recitation?	4.00	yes
Are you often sentimental?	4.00	yes
Do you usually think a great deal before deciding anything?	3.67	yes
Are you ordinarily extremely careful about the friends you make (must know one very thoroughly to call him a friend)?	3.65	yes
Have you usually made friends with ease?	3.61	no
Do you often indulge in self-pity when your luck is bad?	3.61	yes
Have you usually expressed yourself best in writing?	3.59	yes
Are you usually critical of others?	3.45	yes
Do you often act on suggestions quickly, rather than stopping to think?	3.30	no
Do you indulge much in day-dreams?	3.30	yes
Are you a radical (do you want to change the world instead of adapting yourself to it)?	3.29	yes
Can you remember most of the errands and details of your daily routine?	3.18	no
Do you usually talk spontaneously?	3.08	no
Do you usually succeed at selling?	3.00	no
Do you blush readily?	3.00	yes

The responses of all *Ss* in the criterion groups to each of the 140 items in the questionary were tabulated and reduced to percentage values. For example, 94% of the introverts and 36% of the extraverts answered "yes" to item 2 in the personal habits section ("Do you often worry about possible misfortunes?"). The percentage difference is 58, the σ diff. is 0.081, and the critical ratio (diff./σ diff.), 7.02. This item then is truly diagnostic and differentiates between extraverts and introverts.

TABLE II

ITEMS IN THE INTERESTS GROUP HAVING GREATEST DIAGNOSTIC VALUE
(Arranged in order)

	Critical ratio values	Introvert answer
To spend your vacation at some quiet place rather than at a lively resort	6.84	yes
To compete in intellectual contests rather than athletic games	6.42	yes
To be by yourself a great deal	5.80	yes
To work alone rather than with people	5.54	yes
To have quiet amusements rather than exciting ones	4.95	yes
To write a paper on the characteristics of contemporary novels	4.69	yes
To look at pictures of airplanes	4.57	no
To keep quiet when out in company	4.57	yes
To visit an automobile show	4.51	no
To go to social gatherings just to be with people	4.49	no
To take an active part in all conversations going on around you	4.24	no
To work with many people around you	4.21	no
To examine the details of some new kind of machinery	4.18	no
To rewrite letters before mailing them	3.85	yes
To change often from one type of work to another	3.85	no
To listen to a lecture on the history of painting	3.85	yes
To read about airplanes	3.54	no
To confide in others	3.45	no
To look at a window display of carpenter tools	3.30	no
To stay at home during a social affair	3.29	yes
To talk to yourself	3.27	yes
To limit your acquaintance to a select few	3.13	yes
To listen to a lecture on classical music	3.13	yes
To play baseball	3.08	no
To convince others of your point of view (omit questions of religion)	3.06	no
To have people watch you do things that you do very well	3.02	no
To have excitement	3.02	no

Results. Of the 70 items in the personal habits section 25 had critical ratios of 3.00 or better. These items have the greatest diagnostic value and are represented in Table I with their critical ratio values.

Of the 70 interest questions a total of 27 have critical ratios above 3.00. These are given in Table II with their diagnostic values. We arbitrarily take the distance 3 σ as indicating a reliable differentiation between the two groups.

Of the 54 items arranged by Heidbreder in order of diagnostic value,[10] 44 were duplicated in our personal habits section. It was then possible to compute a rank-difference correlation and determine to what degree the items common to both were of equal diagnostic value. This was found to be $+0.35$, which is equivalent to a Pearson r of $+0.36 \pm .09$. It appears that, although both studies list the same items as having diagnostic value, the order of value is not in very close agreement.

Discussion. The items which were most diagnostic in the section on personal habits, as shown in Table I, are quite definitely behavior-segments relating to *subjective* responses. They correspond to what Jung meant when he referred to

[10] Heidbreder, *op. cit., ibid.,* 21, 1926, 120-134; Freyd, *op. cit.*

the subjective determination of attention in the introvert.[11] This was not, as Conklin apparently interpreted it,[12] a concentration on intellectual as opposed to physical activities (this is probably characteristic of Bernreuter's "self-sufficiency"),[13] but is simply a lowered threshold for subjective as opposed to objective experiences. The introvert is characterized by his attention to his own emotions, motives, and trains of ideas. He is absent-minded because his attention is not focused on the world of external reality.

The evidence, however, would not seem to justify the practical identification of introversion with emotional instability, as Bernreuter apparently does when he presents an introversion test correlating +0.95 with neurotic tendency. While the two may coincide in many cases, they certainly are not the same trait of personality.

The items found to be diagnostic in the interests section, as shown in Table II, bear on a radically different type of individual apparently, as we should expect from the fact that the two halves of the questionary correlate only +0.30. The most diagnostic behavior-segments here are those relating to social seclusiveness and to intellectual as opposed to activity interests. Nine of Conklin's 15 items, which are exclusively of the intellectual-active antithesis, are shown to have diagnostic significance.

Not a single item of the subjective-objective classification originally defined by Jung is included in Table II. This indicates that the two traits, introversion and seclusiveness, which so often have been thrown together by writers on the subject of personality, really are quite separate and distinct units.

In pursuit of our belief that seclusiveness was related to Bernreuter's conception of "self-sufficiency" and not to "introversion," we computed rank correlations for 24 cases with the following results: interests score with Bernreuter introversion score, +0.12, with self-sufficiency, +0.32; habits score with introversion, +0.62, with self-sufficiency, —0.30.

We believe, then, that our habits questions, as summarized in Table I, give the most diagnostic behavior-units for differentiating introverts from extraverts along the lines originally laid down by Jung. The interests questions in Table II measure some characteristic which is related to self-sufficiency, and indeed might represent an overlapping of a test of intellectual-active interests and one of social-seclusive behavior. Further analysis of this point is needed.

Summary. (1) Seventy items, selected as alleged introverted habits, showed 25 with diagnostic values of 3.00 or better, indicating the presence of considerable consistency within this group of behavior characteristics.

(2) Seventy items, selected as alleged introverted interests, contained 27 that were successful in distinguishing extreme groups.

(3) Those habits which were diagnostic follow the definition laid down by Jung for introversion-extraversion.

(4) The diagnostic interests were more closely related to self-sufficiency, and were apparently quite distinct from the trait of introversion.

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[11] C. G. Jung, *Psychological Types,* 1923, 471 ff.
[12] Conklin, *op. cit.*
[13] R. G. Bernreuter, Evaluation of a proposed new method for constructing personality trait tests, Thesis, Stanford University, 1931.

APPARATUS

A CAMERA FOR SIMULTANEOUS RECORD OF HORIZONTAL AND VERTICAL MOVEMENTS OF BOTH EYES

By BRANT CLARK, University of Southern California

We have modified the method of corneal reflection first described by Dodge and Cline.[1] A 100-c.p. 6-8 volt lamp overloaded to 10 volts is used as a light source. After passing through a collimating lens, the light is reflected to the corneas by means of two small mirrors. Head-rest and biting-board are used, and a spectacle-frame provided with a specially illuminated chromium-plated ball indicates head-movements.

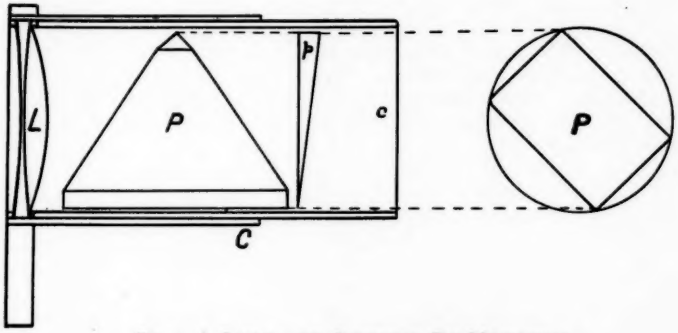

FIG. 1. A CAMERA FOR RECORDING EYE-MOVEMENTS
The lenticular system is shown for only one eye.

The light-beam from each eye is picked up by a focusing lens (Fig. 1, L), a compound achromatic doublet of 275-mm. focus. Beyond the lens the beam is divided into two parts, one central and one peripheral. The lens is mounted in a brass cylinder (C), 44 mm. in diam., by the use of two tightly fitting brass collars. Within this cylinder a smaller cylinder (c) is fitted just loose enough to rotate. Two prisms are mounted in (c). The first (P) is a total-reflecting prism of optical glass, mounted within a light brass housing which is in contact with the apex and the two non-refracting sides and also hooked under the edges of the total-reflecting surface. The housing slips into two tracks in (c) where it is held in position behind the achromatic lens by a long sheet-brass spring which extends the length of the cylinder. The small cylinder is so rotated that the reflecting surface of the prism forms an angle of 45 degrees with the horizontal axis of the instrument. In the same housing a flat prism (p), 0.8 diopter in power, is placed with its base-apex line horizontal.

The resultant effect of this lens-system is as follows. The rays from the peripheral

[1] R. Dodge and T. S. Cline, The angle velocity of eye movement, *Psychol. Rev.*, 8, 1901, 145-157.

part of the lens are focused on sensitized paper without further change. They record horizontal movements as a *horizontal displacement.* The central rays are focused to one side of the peripheral rays, by means of the flat prism; but their motion is rotated 90 degrees by the total-reflecting prism, i.e. *a vertical movement of the eye will also cause a horizontal displacement on the film.* Thus it is possible to record the horizontal and the vertical movements of both eyes simultaneously on a single record. The two lens-systems (one for each eye) are securely mounted in heavy brass mountings with suitable adjustments for independent focusing. The whole mounting is oxidized and lacquered to a dull black finish.

The camera from lens to film is approximately 416 cm. long, giving a magnification of 14 times. The record shows a 1-mm. displacement for each 3-mm. change in fixation at a distance of 30 cm. An episcotister which interrupts the light 25 times a sec., close to its focus, is mounted within, but entirely independent of, the body of the camera.

Sensitized paper[2] backed by light-proof paper is wound on an aluminum drum which takes paper as wide as 6 in. The paper runs through a guide to a lower drum which is turned by a constant-speed motor reduced by a gear-box and variable pulleys. The speed most used gives about four dots per centimeter on the record. The instrument is mounted firmly on three substantial bases heavily bolted to three concrete blocks, 18 x 18 x 30 in., which weigh about 700 pounds each. These blocks reduce vibration to a minimum.

APPARATUS NOTES

Dr. Lauer's 'Improvement' of the Pursuitmeter

In an article recently published in this Journal, Dr. Lauer states that the pursuitmeter "used by Weiss and Renshaw employed a spring handle stylus. This had some disadvantages and a rigid stylus was tried."[1] This statement is in error, for, as a matter of fact, none other than a rigid stylus has ever been used in any experiment we have made with the instrument. We tried out a number of experimental models, among them the spring handle, but we abandoned them for the rigid stylus as soon as it became clear that the tapering off of stylus-pressure constituted one of the most important things a novice had to learn in order to manipulate the apparatus successfully.

Abundant experience has taught us that no other than an inflexible stylus could be employed if the device is to be used for studies on the acquisition of skill. If the apparatus is used as a *test,* then one must measure either how quickly and how much the operator learns, or how much he has learned prior to the first few trials. There is, therefore, no essential difference between the demand made upon the operator when the instrument is used in a learning experiment or as a test device. Hence Dr. Lauer's proposals to use a spring handle stylus and a

[2] Eastman recording-paper No. 697 has been found to be more satisfactory than paper No. 1.

[1] A. R. Lauer, Improvement and standardization of the pursuitmeter technique, this Journal, 45, 1933, 512-515.

ball-point stylus-electrode can only lessen considerably the effectiveness of the instrument.

The ball-point stylus advocated by Dr. Lauer, was used by us in 1923. It was soon abandoned because a ball point permits a large amount of pronation of the hand, wrist, and forearm. In the original description of the apparatus,[2] we called attention to the fact that the arc of the face of the head of the stylus was turned on a precision bench lathe so as to permit not more than 9° of tilt in any direction without breaking the control circuit and stopping the driving motor. The mechanics of the situation is simple. A ball-pointed stylus permits as much as 180° of pronation or tilt and this completely spoils the apparatus either as a learning device or as a test of how much of this particular manipulation anyone has already learned.

We designed this instrument with the anatomy of the arm and hand in mind. Learning consists largely, as our motion picture analysis shows, of a progressive relaxation of the muscles of the arm and forearm, starting with the pectorals and proceeding centrifugally. (This is precisely the same order in which Coghill observed the individuation of movements to follow in the amblystoma larva.) In the human, virtuosity on the pursuitmeter is gained when the learner's movements shift from the jerky, spastic, phasic type in optical-manual pursuit to the smooth, ballistic form in which vision plays only a minor rôle. How this transition occurs is one of the major problems of the psychology of skill. Muscular tension (stylus pressure) and skill are mutually exclusive. Our original stylus was so constructed that it could be held in apposition with the moving electrode with a small amount of pressure, amounting to not more than the relaxed weight of the hand and flexed arm; breaking the control contact was accomplished by any inadvertence in which the stylus head side-slipped in the plane of its movement, or was brought out of parallel with the target. Such a device at once offers many possibilities to the operator, and many problems for the investigator. I would deny that Dr. Lauer has proposed any improvement, save possibly the replacing of the target with the three-hole escutcheon. Even in this case most investigators would probably prefer the Koerth type of apparatus. I question the usefulness of the apparatus as a *test* for such a hypothetical function as "motor skill," particularly where the test score is the number of times a novice breaks the control circuit in the first several trials. Such scores represent the alternate use of several modes of attack by the learner or testee, several varieties of posture or address, and several types of self-imposed instructions added to those given by the experimenter. In such cases the scores are incomparable, either from *S* to *S*, or when made by the same *S* on different trials, for the reason that their validity is zero, regardless of their consistency or reliability.

Ohio State University SAMUEL RENSHAW

A REJOINDER

Due to the courtesy of Dr. Renshaw I have the opportunity of conjoining a reply to his note. I gladly acknowledge that the statement to which Dr. Renshaw objects should have read, "Weiss and Renshaw adopted a rigid handle stylus after experimenting with various types of flexible and rigid devices." My original state-

[2] A. P. Weiss and S. Renshaw, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

ment was based upon information secured while I was working in the psychological laboratory of the Ohio State University. That it was incorrect is due in part to the fact the instrument was variously used during my stay there and also to the fact that Dr. Renshaw mentioned trying out such a stylus. The correction of this error was, however, merely the occasion for a criticism of my article. With many of Dr. Renshaw's points, I find myself in close agreement, which is to say that I regard them as immaterial. With his inference, however, that the rigid stylus is the best type for use in all learning-problems of a sensorimotor nature, and with his argument that the spring device, in studies on the acquisition of skill, defeats the purpose of the apparatus, I find myself in absolute disagreement.

It is possible that Dr. Renshaw's apparatus best suits his problems. On the other hand I have found it unsatisfactory for my studies. When he argues from a different point of view, his remarks are beside the point. When his point of view is similar to mine his remarks are not always germane as he often sets his opinion against my observations. What I attempted to do was to modify the apparatus for a specific use in order to eliminate some obvious sources of error.

Iowa State College ALVHH R. LAUER

NOTES AND DISCUSSIONS

Perception of Depth in the After-Image

In terms of the anatomical view of binocular fusion the images falling on certain parts of the two retinae and these images alone are combined in binocular vision. These areas are called combining or corresponding areas and all other areas non-combining, non-corresponding, or disparate areas. The foveae, for example, are combining areas, also points suitably spaced to the nasal side of the fovea or median plane in one eye and to the temporal side in the other. Some hold to this view so literally as completely to ignore the influence of other important factors in the fusion and the very great versatility of the function. At times the influence of this view leads to rather serious technical blunders and tends in general to act as a handicap in the development of methods of treating the eye.

It is the purpose of this note to caution against a too literal interpretation of the anatomical view and to urge a wider study of the factors contributing to binocular fusion.

One of the requirements of binocular fusion is that the subjective images be referred or projected to exactly the same position or location in space. This includes distance as well as the other two spatial dimensions. In this connection it is scarcely necessary to point out that space reference or projection is very intimately connected with the motor mechanism of the eye. The foveae of the two eyes, for example, combine their images only with convergent lines of regard. Parallel lines of regard would not cause the superimposition of the two projected, subjective images. Space will not be taken here, however, either to review the evidence for the importance of the rôle played by the motor mechanism in space reference or to present results on the factors in binocular fusion. The latter will be made the subject of future papers.

It is our special purpose in this paper to describe conditions under which images on disparate portions of the two retinae are combined. In the customary way of viewing objects at different distances the images of the object fixed or located at the intersection of the lines of regard fall on the foveae of the two eyes and combine. The images of nearer objects which fall to the temporal side and of farther objects which fall to the nasal side of the two foveae do not combine and the objects are seen as doubled. The avoidance of double images is one of the eyes' strongest reflex incentives to movement. The movement resulting from this incentive is convergent for objects nearer and divergent for objects farther than the plane of fixation. These movements not only bring the images to the foveae of the two eyes, which causes their combination, but also place the eyes in that relation to the object which gives the clearest possible perception of image and approximately the clearest possible formation of image. A threefold service is thus rendered in the interests of clearness of seeing; also an important service in the economy of vision in the third dimension. That is, under the influence of these incentives the eyes, unless restrained, automatically converge and diverge and the point of fixation travels from far to near and from near to far. So quickly and effectively is this accomplished that the details of a solid object, even of several objects at different distances seem to be seen at a glance and the change in the adjustment of the eyes to pass unnoticed.

In this, however, there is nothing contradictory to a literal interpretation of the anatomical view of binocular fusion. The convergent and divergent movements of the

eyes bring the images to the appropriate combining areas. When we look at a segment of a cone, for example, and direct our fixation to its front, the back doubles. This results in a diverging movement of the eyes to bring the images of the back to the foveae, which causes it to be seen as single and the front to double. The doubling of the front in turn causes a converging movement to recombine the images of the front, and the back doubles; and so the process goes on until the fixation is confined to some particular point by conscious interest or voluntary effort, or is directed elsewhere.

In case we view the after-image of the cone, however, the situation is quite different. The basis of the image here is a physiological excitation which continues after the physical image has been removed. If, in forming the image, fixation is directed to the front of the cone, the images of the front are superimposed on the two foveae but the images of the back are displaced towards the nasal side and thus fall on disparate areas. Now when the physical images are removed and we have remaining only the physiological images, obviously no amount of diverging movement can transfer these images to the foveae. Correspondingly it is commonly said that depth or solidity can not be obtained in the after-image. This conclusion, however, as often happens, is based on a too limited trial of conditions. After-images are usually projected for observation on a surface or background of some kind. Under these conditions depth is not obtained; but if they are projected under conditions which place no restraint or control upon fixation, in other words upon the convergent and divergent movements which have always been associated with the viewing of the object, full and natural depth is obtained. A convenient way of securing these conditions is to observe the after-image in the darkened field of view. When the after-image is projected into the darkened field of view the eyes are free to react to all the incentives for movement produced by the placement of the physiological images or impressions on the retinae and it seems that nothing more than these movements or the incentives to movement, is required to cause both the fusion of the images located on the disparate parts of the two retinae and the natural and full binocular perception of depth.

The result may be made clearer perhaps by a description of the conditions under which the observation is made. A segment of cone, for example a small megaphone, is surfaced in two colors to differentiate between the front and the slant surface. The slant surface may, for example, be covered with red paper and the opening in front with blue paper, with a narrow border of white to give still better definition. When this is viewed from 40 to 60 sec. and the after-image is projected on a sheet of cardboard or other plane surface, the following phenomena occur. When the front of the cone is fixated in securing the stimulation for the after-image, its images are centered on the foveae of the two eyes and the circles which represent the back of the cone are imaged to the nasal side of the two foveae. These circles, it should be noted, are filled by the red of the slant surface. In the after-image the front of the cone is seen as a single image, a yellow disk. The images representing the back of the cone are seen as doubled, *i.e.* as two overlapping disks of green. As is always the case for images which do not superimpose, these disks rival in their tendency to hold the field, *i.e.* sometimes both are present and at other times one and then the other. When the observation is made under the converse conditions of fixation, the green disks representing the back of the cone are combined in the after-image and the yellow disks representing the front of the segment are seen as doubled and as rivalling

for the control of the field. When the after-image is projected in the darkened field of view, however, the cone stands out vividly in space in full and natural binocular depth. When the cone is viewed in a strong light, preferably daylight, in order to secure an after-image of good intensity, the result is quite striking. The effect is that of a luminous cone, with an orange front and a blue-green slant surface, hanging in space without visible means of support or suspension. Just as in viewing the object the depth is better seen if the cone is viewed from a point a little above and to one side, so in the after-image the depth will be better seen if in securing the stimulation the cone is viewed from a point above and a little to one side. So far as we have been able to determine, fusion and depth may be obtained in the after-image under any conditions of projection or observation which do not control fixation or place any restraint on the convergent and divergent movements of the eyes. For example, it can be obtained when instead of in the darkened field of view the image is projected towards a dull sky on a suitably dark day. Under these conditions the lines of regard do not become parallel as is often supposed but assume the natural converging position which they take when there is no control of fixation. That is, the conditions with reference to the direction and control of the lines of regard are very much the same as they are in the dark room.

With reference to the above conditions of observation the situation may be summarized as follows: (1) when the appropriate motor factor is present and the images are on combining portions of the two retinae, fusion occurs; (2) fusion also occurs when the appropriate motor factor is present and the images are on disparate parts of the two retinae; but (3) it does not occur when the images are on disparate parts of the retinae and the appropriate motor factor is absent. Under these and other conditions of observation which are entirely correct and proper, a strong case may be made for the importance of the motor factor. In the normal seeing of objects both factors are present and there is nothing to indicate whether or not both are essential to fusion or to differentiate their relative importance as fusion factors. In a later paper the importance of the motor factor will be further shown.

Other ways of demonstrating that images on disparate parts of the two retinae may fuse and will fuse when no control or restraint is placed on the eyes' motor reactions to the placement of their images and further that depth may be obtained in the after-image, may be cited without going very far afield. One of these we have frequently used in connection with those given above in class-room demonstrations. The observer stands at an open window facing a brilliantly illuminated landscape or better still an enclosed space such as a cloister and covers his eyes for 10 min. or more with a black cloth to render them suitably sensitive to light. The cloth is then removed for 1 or 2 sec., the illuminated space viewed, and the eyes re-covered. There soon develops in the darkened field of view a clear reproduction of the scene viewed in full and natural binocular perspective. In this case only the images of the objects in the plane which contains the point of fixation in obtaining the exposure fall on the combining portions of the two retinae. The images of all objects located in planes nearer and farther than the point of fixation fall on disparate areas, yet perfect fusion occurs at all points and full binocular depth is present in the visual impression.

Whatever is to be the ultimate view of binocular fusion, it has at least been shown here that images formed on what are often and perhaps usually considered as disparate parts of the two retinae may fuse. In this connection it should be remem-

bered too that one of the essential conditions of fusion is that the two images be projected exactly to the same position with reference to all three spatial dimensions and that the motor factor both directly and genetically is regarded as intimately connected with all space reference or projection. The evidence which we have thus far been able to compile seems to indicate that binocular fusion is the resultant of not one but several factors and that there is a very considerable interplay of influence among these factors.

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EYE-MOVEMENTS AND THE PHI-PHENOMENON

Photographic records, taken of the eye-movements during the successive presentation of two lights, indicate that the eyes do not move as often during the experience of phenomenal movement as during the intervals when movement is not perceived.

The eye-movements were recorded by photographing the corneal reflection from the right eye. The photographs were taken with a Cirkut panoramic camera which was adapted so that an entire 12 exposure ($2\frac{1}{2} \times 4\frac{1}{4}$ in.) film could be unrolled downwards past the exposure-field for each record. The corneal reflection was focused to a small point of light so that it left a narrow line along the length of film; abrupt deviations to the right or left indicated eye-movements in the horizontal plane. The camera was further adapted so that the film was exposed to a second point of light whenever *O* pressed a key signalling the occurrence of an experience of phenomenal movement; the key was held down as long as the movement experience lasted; this left a second line, usually an intermittent one, along the length of the film. Also the film was exposed to a third point of light which coincided with the presentation of the right hand stimulus-light; these flashes left a row of dots along the film. Thus one could read across the film and see the temporal relation between the stimuli, the eye-movement, and the presence or absence of phenomenal movement.

The stimulus consisted of two lights placed in a horizontal relation, 50 cm. apart, and 300 cm. from *O*. Each stimulus consisted of a flashlight bulb exposed to *O* through a circular hole, 0.5 cm. in diam. This was covered with a No. 20 bond paper so that an even brightness without glare was obtained. The lights flashed on in a uniform succession of alternate exposures. The duration of each flash was 50 σ and the interval between the alternate stimuli was 200 σ. A series of 83 alternations, on the average, was recorded on each film. The *O*s, 11 in number, were given preliminary presentations of the stimuli so as to become familiar with the experience of phenomenal movement; at the same time they became adjusted to the position for observation with the head placed in the head-rest and the right eye submitted to the light source of the corneal reflection, a beam from a 100-watt lamp filtered through blue glass. When the *O*s could make the distinction between full movement and end movement they were instructed to press and hold the signal key down only when they perceived full or nearly full movement. The *O*s were given no instruction regarding the direction of their visual regard or their fixation. When the photographic records were taken the camera was set in motion at the moment when *O* first signalled that he was perceiving movement.

Table I shows the average number of eye-movements for every *O* per inch of

film, during the periods when movement was reported and also during the periods when movement was not perceived.

TABLE I

SHOWING FOR EVERY *O* THE AVERAGE NUMBER OF EYE-MOVEMENTS MADE DURING INTERVALS WHEN PHENOMENAL MOVEMENT WAS PERCEIVED, AND WHEN IT WAS NOT PERCEIVED

O	Average no. eye-movements when movement reported	when movement not reported
Br	.40	.77
Go	.45	.46
Gu	.22	.09
Ha	.66	.69
Ho	.27	.30
Hu	.62	1.22
Ki	.19	.84
Sa	.56	.87
Sc	1.03	.82
St	.08	.18
Tr	.75	1.44
Av.	.47	.70
Diff.	0.23	
σ diff.	0.14	

Table I shows that for 6 *O*s eye-movements occurred more often during the periods when phenomenal movement (*phi*) was not perceived; that, for 2 *O*s, more eye-movements occurred during *phi;* and that, for 3 *O*s, the number of eye-movements was practically the same during the two periods. The difference between the averages for all *O*s is 0.23 (S.D. 0.14) in favor of the movements during which phenomenal movement was not perceived. The eye-movements recorded in Table I varied in magnitude from the angle formed by the distance between the stimuli (nearly 9°) and movements which were just perceptible. (Tremors were not recorded.) Some of the movements appeared to follow the stimulus-lights back and forth, others appeared to jerk or to snap over. There were just as many movements, however, that varied in an irregular manner.

In a second series of experiments the *O*s were instructed to follow the lights with their eyes, *i.e.* to fixate each of the stimulus-lights in turn as it flashed on. Preliminary trials showed that *phi* was not perceived during these voluntary pursuit movements. In further trials when the pursuit movements had been in progress for 15 sec. the *O*s were told, "Now try to perceive *phi!*" The photographic records showed that in all cases the large pursuit movements ceased immediately upon the instruction to perceive *phi,* and only small indefinite eye-movements remained. Also, an interval of time elapsed (varying in length for the different *O*s) before the appearance of phenomenal movement was reported.

In a third series of experiments a comparison was made between two points of fixation during the presentation of the alternate stimuli. Tiny cross-shaped perforations illuminated from behind were used as fixation-points. One fixation-point was located on a horizontal and transverse line midway between the stimulus-lights; the other point was located 200 cm. beyond the stimulus-lights, and was visible to *O* at a point just above the nearer fixation-point. The *O*s were to report the perception of phenomenal movement by pressing a key which actuated one of the pens of a (Foote,

Pierson) double pen writing recorder; with the other pen *E* indicated which fixation-point was being used. Observations were commenced in some cases with the nearer fixation-point and in the other cases with the further one; in both cases after 30 sec. the alternate fixation-point was presented in place of the one used. In both situations the frequency and duration of phenomenal movement was about equal. From casual comments by the *O*s there had been some indication that phenomenal movement was perceived better if the eyes were fixated beyond the stimuli. But this suggestion was not supported by the records.

The presence of a fixation-point often evoked autokinetic sensations which involved the entire pattern of stimulus-lights and fixation-point. Several *O*s who had perceived *phi* in the previous experimental series now reported (verbally) the autokinetic sensation during a greater portion of their observations. Of 12 new *O*s who had no instruction or experience regarding the *phi*, 6 reported autokinetic sensations predominantly, and the others built up a perception of phenomenal movement.

These experiments, which give results corroborating Guilford and Helson,[1] are offered as a study of the overt oculomotor behavior in relation to the phi-phenomenon. They are not considered to be a test of the presence of incipient eye-movements nor to be an indication of any other type of bodily response which might be related to the experience of *phi*.

Princeton University

WILBUR S. HULIN
DANIEL KATZ

THE REALITY OF BRIGHT AND DULL PRESSURE

Much of the criticism directed against the doctrine of bright and dull pressures seems to center around the feeling that these pressures are esoteric introspective artifacts, producible only under definite laboratory bias and existing only in the imagination of their adherents. It would seem to the present writer that this view may be attributed in part to the tendency to regard these pressures as new sensory elements fitted into Titchener's touch pyramid. However, the reports of the writer's own *O*s indicate that these pressures are not novel elements of experience but simply the "complex sensory resultant of the general bodily adjustment involved in the affective response."[1] The most significant thing about the response of these *O*s was their recognition of a real bodily experience *localizable* in the trunk and in general answering to the description of bright and dull pressure evolved in Nafe's original work.[2] Apparently they believed that this *experience* was of primary importance and that the terminology was a matter of convenience and communication only. As one of the original *O*s has since remarked to the writer, Nafe's description never completely satisfied him, *but* he has been unable to substitute anything better. Titchener himself has frequently mentioned the language difficulties involved in introspection and cautioned against allowing this difficulty to blind us to the experiential reality lying behind the report.

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[1] J. P. Guilford and H. Helson, Eye-movements and the phi-phenomenon, this JOURNAL, 41, 1929, 595-606, esp. 599.

[1] W. A. Hunt, The meaning of pleasantness and unpleasantness, this JOURNAL, 45, 1933, 347.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

As any laboratory experimentation involving the direct influence of the present writer would be open to the charge of "laboratory bias" he settled on the questionary method as a possible means of further work. Although such results can not be considered conclusive, they are, nevertheless, interesting, indicative, and suggestive.

In the introductory psychology class at Dartmouth 109 members who were unfamiliar with the field of affectivity, were given the following questionary. They were not students of the writer, nor did he administer the questionary.

"It has been said that pleasantness is associated with a 'bright pressure,' a touch quality lying between a bright tactual pressure and tickle. It is a diffuse bodily feeling with expansive qualities, resembling expansiveness of the body, and is located in the chest.

"Unpleasantness has been associated with a 'dull pressure,' a touch quality lying between tactual dull pressure and strain. It is not extended and has qualities of constriction, resembling bodily contraction. It is located in the abdomen, or low down in the chest.

"Consider the above statements in the light of your own experience and check your opinion of them on the following scale:

"True Probable Improbable Not True

"Remarks (add any remarks you may care to)"

The questionaries are tabulated, as collected in four groups to demonstrate the uniformity of the sampling. The remarks gave every indication that the questionary was taken seriously and with consideration.

The results show 26 judgments of *true*, 60 of *probable*, 16 of *improbable*, and 7 of *not true*. This would seem to indicate that the doctrine strikes a responsive chord in the majority of the group. It is interesting to note that the remarks on the "improbable" and "not true" questionaries deal with points of difficulty previously noted in the literature. Objections were raised on the grounds that P and U were intellectual and not sensory, that the pressures were only noticed in extreme cases of P and U, and that they were not definitely localizable. A few cases were found where the dull pressures were admitted, but the bright pressures were either denied or called hardly noticeable.

As Young, opposed as he is to Nafe's claims, has generously suggested, "It would be unfortunate if the present discussion obscured genuine problems."[3] The writer feels that genuine problems are being obscured; that in the quarrel over particular terminology and systematic interpretation we are losing sight of an experiential reality, that behind Nafe's particular description lies an important experience that offers a vital approach to the physiology of affectivity, and that Nafe's investigation may well be the beginning of work which will ultimately bring the quest for bodily correlates of pleasantness and unpleasantness to a successful conclusion. With this in mind he tried a new questionary, similar in form, but containing a simplified, common-sense statement as follows:

"It has been said that pleasantness is associated with a light expansive feeling in the chest, and unpleasantness with a heavy, contracted feeling in the 'pit of the stomach.' "

This was given to further group of 62 introductory students at Dartmouth, 94 introductory students (during their first week's contact with psychology) at Connecti-

[3] P. T. Young. The relation of bright and dull pressure to affectivity, this JOURNAL, 44, 1932, 784.

cut College for Women, and 54 adults outside the laboratory. This last sample was supposed to represent the mythical man in the street and ranged from two negro elevator boys to a professor of English. The distribution within each group was quite comparable so the results may be pooled.

The results showed 89 judgments of *true*, 91 of *probable*, 10 of *improbable* and 20 of *not true*. Here again the doctrine seems to strike a responsive chord in naïve experience. One can only ask: why?

Connecticut College for Women WILLIAM A. HUNT

A Case of Environmental Change with an Accompanying Loss of Language

Cases illustrating rapid forgetting with major environmental change, such as that reported by Carr,[1] are not frequently found in the psychological literature. The following case seems, therefore, worthy of notice.

As elicited in a personal interview with the mother on September 27, 1933 the main facts are these: Katherine, as we will call her, was born in Fayetteville, June 2, 1928. Her parents are Dutch, natives of Holland. Katherine learned and spoke excellent English (a fact substantiated by several witnesses), never used the Dutch, although her mother thought she could understand it. On April 10, 1933, Katherine and her mother arrived in Holland. An incident which occurred five days later will show how dependent Katherine was upon the English on her arrival. On the fifth day of her stay her grandfather took her for a short walk. In about an hour he brought her home. Katherine was crying and her grandfather was distracted. The child had asked to have her shoes and stockings removed (in English) and grandfather could not understand her. By the tenth day of her stay Katherine was speaking a South-African dialect, a Dutch-English mixture. By the fourteenth day, the mother says, "Katherine spoke more and better Dutch than I do English now." (The mother has been in Fayetteville six years.) In three weeks time after her arrival Katherine started to nursery school and suffered no language difficulty in her work or associations. She spoke Dutch exclusively and asked her mother "not to talk that funny way (*i.e.* in English) any more." On July 29, 1933, Katherine's father arrived in Holland and greeted her in English. Since Katherine did not seem to understand him he was compelled to use Dutch. Other incidents also emphasize the extent to which she had lost her command of English: stories and nursery rhymes learned in English were forgotten or at least could not be repeated in English. So thoroughly had she learned the Dutch that on a visit to Rotterdam she could speak the pure Dutch instead of the dialect used in her grandmother's home in Zierikzee.

The family left Holland on September 9, 1933. On the return trip a stewardess who had known and talked with Katherine on her way over in April attempted to talk to Katherine again in English. Katherine did not understand the stewardess until she spoke Dutch. The family arrived in Fayetteville on September 22. A day or so later Katherine went to play with one of her former chums. Katherine would become angry with him because he could not understand Dutch and he with her because she no longer spoke English. Her mother told the writer that on two occasions, September 24 and 26, Katherine had asked her parents to tell one of her playmates in English what she was trying to tell him in Dutch. During the writer's interview Katherine did not speak to him, partly on account of shyness no doubt,

[1] H. A. Carr, *Psychology*, 1925, 251-252.

but also because she had not then relearned the English. She did, however, speak frequently with her mother in Dutch.

The child was seen almost daily from September 27 to October 3 by an assistant,[2] who also visited other friends of the family who knew the child and corroborated all the above items. They also informed us that upon her arrival in Fayetteville she did not know the English words for eyes, nose, or mouth although she readily gave the Dutch words. Katherine rapidly reacquired the use of English. In doing so she first used monosyllables only, incomplete sentences, etc., just as any child does who is first learning to speak. By October 3, 1933, she was playing with her chum and was speaking English almost entirely and apparently understood everything said to her in English.

Briefly, then, we have here the case of a five-year-old girl who lost the use of English after two weeks time spent in Holland (where she heard no English whatsoever) and who learned Dutch during that time. In about the same period of time spent in an English-speaking environment she has reacquired English. She still retains the Dutch since her parents are now using that in the home in the hope that she may not lose it. On October 31 her Binet mental age was five years, eight months.

One or two items give evidence of the fact that not all knowledge of English had vanished in Holland. In first speaking the South-African dialect she built her sentences according to the syntactical rules of English though this was not true when she later spoke Dutch. She used an English accent on her Dutch words. (Here it might be recorded that on October 31, at the time of her test, she used some Dutch accent which was not true of her English before she went to Holland.) Again she might start off with the first few words of familiar nursery rhymes in English, would be seemingly unable to proceed in English, but would continue with a sort of Dutch translation of the poems.

It should be recorded also that the child made a brief visit to Holland when she was two years of age and at that time started 'baby talk' in Dutch which she dropped as soon as she left. Up until this time her parents had talked Dutch almost entirely, Katherine's mother was just learning English at the time, but since then very little Dutch has been used in the home. It is these last items that make Katherine's case not as clear as might be wished from the standpoint of total loss of English and acquisition of Dutch *de novo* but the fact of major loss is nevertheless clearly shown.

University of Arkansas R. H. WATERS

THE SAVING SCORE AS A MEASURE OF RETENTION

The saving score originated by Ebbinghaus[1] continues to be used as a device for expressing results in retention experiments. The score is usually given as a percentage representing the ratio of time or repetitions saved in relearning to the time or repetitions required for original learning.

An ambiguity arising in the use of the saving score has puzzled many of the experimenters using this measure, but there is to my knowledge no clear discussion in the literature of the alternatives involved in calculating the score. The problem

[2] Miss Rachael Jones assisted in this part of the investigation.

[1] H. Ebbinghaus, *Über das Gedächtnis*, 1885. (Trans. by H. A. Ruger and C. E. Bussenius, 1913, 1-123.)

arises out of the inclusion or exclusion of the test trials in computing learning and relearning. There are three possibilities: (1) the test trial may be included both in learning and in relearning;[2] (2) the test trial may be omitted both in learning and relearning; and (3) the test trial may be included in learning, omitted in relearning.

The results obtained by using each of these three methods may be illustrated in a simple table. The exact values are unimportant. Suppose that in learning by the anticipation method to a criterion of one perfect repetition there are required six trials, including the final criterial trial (perfect anticipation). In a retest (A) the first trial yields a perfect recitation (maximum possible saving); in another retest instance (B) relearning requires six trials (there is no saving). Under the three types of scoring the savings given in Table I result.

TABLE I

SAVING SCORES FROM AN EXAMPLE OF MAXIMUM SAVING AND OF ZERO SAVING AS CALCULATED BY THREE METHODS

Test trials	(A) Maximum saving			(B) Zero saving		
	Trials required		Saving score	Trials required		Saving score
	Learning	Relearning		Learning	Relearning	
1. Included	6	1	83%	6	6	0%
2. Excluded	5	0	100%	5	5	0%
3. Included in learning, excluded in relearning	6	0	100%	6	5	17%

It is evident from the table that the identical experimental data may yield a variety of scores for maximum and minimum saving. The only score which gives a maximum of 100% and a value of 0% for no saving is the second, that which *excludes* the test or criterial trials. Either alternative (1) or (3) will give results at one extreme which depend on the number of trials required for original learning. Uncertain maxima or minima are undesirable aspects of any scoring scale. The differences between the several scoring devices are accentuated when the learning trials are few in number. For more difficult materials, requiring a larger number of trials, the scores are less affected. The second score, the one used by Ebbinghaus, would seem to be the one which ought to be recommended for general use as the standard for saving scores. Finkenbinder,[3] who introduced the anticipation method, apparently used the first procedure suggested above, not correcting for criterial trials. Luh,[4] whose methodological study has served as the basis for much later experimentation, is explicit in adopting the Ebbinghaus procedure.

The matter could be permitted to rest here were it not that there is learning during the test trial in the original practice, and subtracting this trial seems to do violence to experimental fact in order to conform to a scoring device. McGeoch states the situation thus:

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[2] In the anticipation method the test trial is the final criterial trial, during which there is perfect anticipation of the items which are presented.

[3] E. O. Finkenbinder, The curve of forgetting, this JOURNAL, 24, 1913, 8-32.

[4] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

"It is obviously necessary to take into account all antecedent practice, whether criterial or not, but it is not necessary to include the criterial trials of the particular relearning because the material is assumed to have been relearned by the time the criterion can be met and the criterial trials may be thought of as tests of relearning."[5]

McGeoch is here favoring the third type of score, which Table I shows to yield a possible 100% maximum saving, but an uncertain minimum score, depending upon the trials required for original learning. He is aware of the difficulties involved in the use of any one of the scores, and has indicated that scores calculated by alternative methods yielded results similar to those he reported.[6]

The procedure of subtracting test trials, with its definite 0% and 100% possible saving, should always be used, except under circumstances in which the experimenter, following McGeoch's practice, makes explicit his reasons for using another scoring device.

Stanford University ERNEST R. HILGARD

THE THEORY OF FORGETTING

The experiments of Jenkins and Dallenbach on "Obliviscence during Sleep and Waking"[1] have indicated, in the words of these authors, that "forgetting is not so much a matter of decay of old impressions and associations as it is a matter of interference, inhibition or obliteration of the old by the new." The great theoretical significance of these experiments has now received widespread recognition. The present writer wishes to call attention to certain evidence which furnishes a very impressive corroboration of Jenkins and Dallenbach's thesis, and at the same time permits it to be given a somewhat more specific form.

Störring[2] has recently described a unique psychopathological phenomenon which is of capital interest from the standpoint of general psychology—a complete and isolated defect of immediate memory. As a result of gas poisoning, which presumably caused some organic change in the brain tissue, the patient studied by Störring has entirely lost the ability to retain any fresh impressions. He manifests no abnormalities of behavior which cannot be referred to this single specific defect. He is able to answer concise questions intelligently, but cannot carry on a protracted conversation. By the time he has finished speaking a sentence of any considerable length, he has forgotten what he was talking about. All persons whom he has met since the time of the accident remain strangers to him; and all events which have happened since then (even such important events as his own marriage) have been completely forgotten. He knows nothing about his memory-defect or about the accident which caused it.

On the other hand, he has an excellent recollection of events previous to the accident, and his behavior in every respect shows that the experiences acquired under normal conditions have not been lost. Störring's observations were made nearly five years after the time of the accident. The patient was still able to recall the

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[5] J. A. McGeoch, The comparative retention values of a maze habit, of nonsense syllables, and of rational learning, *J. Exper. Psychol.*, 15, 1932, 676.

[6] *Ibid.*, 669.

[1] J. G. Jenkins and K. M. Dallenbach, this JOURNAL, 35, 1924, 605.

[2] G. E. Störring, Über den ersten reinen Fall eines Menschen mit völligem, isoliertem Verlust der Merkfähigkeit, *Arch. f. d. ges. Psychol.*, 81, 1931, 257.

events of the day preceding the accident, in much the same detail that a normal person would recall the events of yesterday. If asked, "What day is this?" he would invariably name the date of the accident. Events which happened in the few weeks preceding the accident were also accurately remembered.

Although Störring is apparently unaware of the work of Jenkins and Dallenbach, he arrives at a very similar conclusion: "It is not time in itself, but the impressions occurring in time, which normally contribute to the forgetting of old impressions."[3]

Störring goes on to suggest that, in the case of the normal person, each recall of an earlier experience leaves a new trace of its own, and that these new traces somehow interact with the old trace and tend to blur it away. However, the well-known facts of retroactive inhibition indicate that no such specialized hypothesis is required; almost any sort of mental activity, it would seem, has a tendency to destroy the memory of earlier experiences. Jenkins and Dallenbach's results certainly point very conclusively in that direction.

Störring's case, however, throws some new light on the question of the *temporal locus* of retroactive inhibition. The indication is that normal forgetting depends not on the mere occurrence of new activities as such, but rather on the occurrence of new activities which themselves leave *after-effects* or traces. Forgetting (in other words, retroactive inhibition) is not due to an immediate influence of a present activity upon a memory-trace, but to a later interaction between one trace and another.

After an interval of sleep, as Jenkins and Dallenbach showed and as has been verified by Van Ormer,[4] there is less forgetting than after an equal interval of waking life. It is possible that the low retroactive effect associated with a period of sleep depends upon the joint presence of two factors: a low level of activity, and an instability of the *new traces* produced by such activity. Immediate memory is poor in the hypnagogic state, and it is notorious that dreams are easily forgotten. Activities occurring during sleep which left no new traces behind would presumably have no retroactive influence.

University of Kansas D. M. PURDY

Howard Crosby Warren: 1867-1934

Dr. Howard C. Warren, Stuart Professor of Psychology at Princeton University and head of the Department of Psychology until 1931, died after a few days' illness in New York, January 4, 1934. He was one of that group of American pioneers in experimental psychology which includes Titchener, Cattell, Angell, Sanford, and Witmer. The influence of his frank and generous personality, of his modesty, courage and purposiveness, has been felt by several generations of psychologists, both in America and abroad. He was a graduate of Princeton University and devoted his entire professional life so untiringly to that institution that his name is indelibly associated with Eno Hall and Princeton psychology.

Howard Crosby Warren was born at Montclair, New Jersey, June 12, 1867. An injury at the age of eighteen months necessitated much medical attention in America

[3] *Op. cit.*, 303.

[4] E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49.

and Europe and seriously interfered with his preparatory education, which was very irregular. That he was able to enter Princeton in 1885 is proof of that quiet courage and strength of character which were so much in evidence throughout his life. In 1890 he was appointed an instructor in logic and came into close contact with James McCosh. There followed several years of study abroad. On his return to Princeton, he was appointed a demonstrator in psychology under J. Mark Baldwin. His advancement was steady. In 1904 he was made Director of the Psychological Laboratory and in 1914 Stuart Professor of Psychology. The development of psychology at Princeton was greatly retarded by its close relation to philosophy, as the teachers of this latter subject had overwhelming influence in departmental affairs. Through courage and persistence he was able, after many years' struggle, to obtain in 1920 a separate department of psychology, and he was named its first chairman. Due to his efforts and in part to his financial support, a separate building, Eno Hall, was erected for experimental psychology in 1924. He withdrew from the directorship of the Laboratory in this year in order to devote more time to his literary tasks, but he retained his offices in the laboratory building and, as chairman of the Department, continued to give unsparingly of his time and advice regarding the advancement of psychology at Princeton.[1]

The development of his thought and the influences underlying it are easy to trace. Due to his inability to take much exercise, he had abundant time for reverie and speculation and he began early to question dogmatic belief. This critical attitude and dislike of the purely conventional became a striking feature of his character. Combined with this attitude was his hatred of false values, sham and cant. The puritanical ideas and mysticism of his early youth gradually gave way to a mechanistic view, which persisted to the end of his life. The Darwinian theory appealed to him early in his career. During his college days he became interested in Spencer, Huxley, Clifford, and Tyndall. He found the psychological teachings of James McCosh interesting—most probably his introspective nature, habit of self-analysis, and emotional control made him particularly receptive to problems of the mind,—but Spencer and the associationists already appealed to him much more than a Scotch Presbyterian interpretation of mental processes. When he went to Europe after graduation, he fortunately came in contact with Ebbinghaus, Stumpf, and Wundt, and he was definitely won over to experimental psychology but not to the theoretical doctrines of Wundt, which evidently appeared too mentalistic to the youthful scholar. The psychophysiological aspects of Münsterberg's action theory made a strong impression on him. He was also induced to accept the concept of the nervous arc through the arguments and controversy about feelings of innervation, which was at the time the battlefield of psychologists. And so at this early stage of his development, his general point of view was formed. Although he was tolerant of the views of others, it is evident from his writings that he never changed. This tenacity of purpose toward the things which seemed to him really worth while was his most outstanding characteristic in matters both intellectual and moral.

In the early years of his academic career he published the results of experiments in memory, rotation, etc., but there are relatively few experimental papers in his bibliography, which consists to a very large extent of theoretical discussions of funda-

[1] For a portrait of Professor Warren see frontispiece of this number (facing page 189); for a more complete account of his life, see his autobiography in *A History of Psychology in Autobiography*, I, 1930, 443-469.

mental problems. He was by temperament empirical and was constantly making observations of human and animal behavior, but his physical handicap prevented him from obtaining as practical a knowledge of apparatus and laboratory technique as he would have liked.

As we have seen, he was interested from the outset of his studies in the English school of psychologists and he published a number of papers in this field of psychological theory, but it was not until 1921 that his *History of the Association Psychology* appeared. Shortly before, he published a textbook, *Human Psychology* which, being not quite suited to introductory courses, he developed into his *Elements of Human Psychology,* a book which made a wide appeal largely on account of its semi-behavioristic point of view, its concise statement of essential points, and the absence of extreme views on controversial subjects. He believed that what distinguished the various contemporary schools of psychology was their different interpretations of only partly known facts. He was greatly influenced by the movement which culminated in Watson's radical behaviorism, but he believed that every method available should be used in investigating the phenomena which are broadly included under the name of psychology. He was, therefore, a strong believer in introspection, with a leaning toward behaviorism. He was a dogmatic advocate of determinism. His early flight from mysticism made any theory which verged on vitalism or teleology, such as current purposive psychology, extremely repellent to him. He thoroughly believed in consciousness, but as he himself expressed it "neither awareness nor any of the complex forms of conscious experience, guides or modifies in any way the course of responsive activity. The form and direction of the response is fully determined by neural conditions, except in so far as external forces hamper or promote its completion."[2] His insistence on a neurological explanation of all mental processes follows from his double-aspect theory of the relation of mind and body, which he thoroughly discussed in his presidential address before the American Psychological Association.[3] He also emphasized the importance of response, but he never entirely agreed with the motor theorists any more than with the behaviorists. For him the sequence of events is stimulus—consciousness—response.

He rendered inestimable service to psychology by developing the *Psychological Review* publication.[4] As editor of the *Psychological Review,* he encouraged any honest, serious, and intelligent attempt to solve the important theoretical problems of the science.

Throughout most of his life he was interested in words and definitions. In 1915 he was appointed chairman of the Committee on Terminology of the American Psychological Association. At the time of its discharge in 1924, the grand total of completed definitions had only reached seventy-nine terms. He therefore set out to write a dictionary of psychology. This project occupied most of his attention during the last years of his life. It was practically completed before his death, and will undoubtedly stand as a reminder to future psychologists of his untiring service to his colleagues and to his chosen science.

Princeton University H. S. LANGFELD

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[2] H. C. Warren, Outline of a psychological standpoint, this JOURNAL, 39, 1927, 34.

[3] Warren, The mental and the physical, *Psychol. Rev.,* 21, 1914, 79-100.

[4] For his relations to these journals, see the account by S. W. Fernberger, Howard Crosby Warren: 1867-1934, *Psychol. Bull.,* 31, 1934, 1-4.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Psychology of Pleasantness and Unpleasantness. By J. G. BEEBE-CENTER. New York, D. van Nostrand Co., 1932. Pp. viii, 427.

The purpose of the book is "to remedy the ghost-like state of the psychology of pleasantness and unpleasantness by bringing its facts together into a single orderly structure." This purpose is well accomplished through the survey of experimental work upon pleasantness and unpleasantness and the presentation of an organized view of the subject.

Beebe-Center distinguishes clearly between the actual affective experience and the concept of hedonic tone. The concept defines a quantitative variable with positive and negative phases and a range of indifferent values centered about zero. Zero value is not distinct from P and U but an ideal boundary between them. After considering the terms 'affection,' 'feeling,' 'affectivity,' 'feeling tone,' 'affective tone,' the term 'hedonic tone' is adopted as the one best suited to describe the algebraic variable in question. The question whether the P and U *experiences* are quantitative is ruled out as metaphysical.

The second chapter of the book is an exceedingly useful summary of the methods of experimentation in affective psychology. Starting with a classification of methods by Külpe, the discussion follows the traditional distinction between methods of impression and expression. From Fechner's method of choice have developed the three classical methods of impression: (1) order of merit; (2) paired comparison; (3) single stimulus, or single exposure, which includes the scale of values and the 'percentage of pleasantness' methods. The methods are discussed in detail with recent modifications and criticisms.

The reviewer notices the omission of the method of qualitative analysis, *i.e.* the description of qualitative, intensive, and temporal changes of affective experience. The explanation for the omission is doubtless that the concept of hedonic tone disregards the affective experience as such. Nevertheless the term "hedonic tone" is used in many places throughout the book as if referring to an affective experience rather than to a concept. Even the word "expression" implies a dualism between affective experience and bodily process.

In Chapter III Beebe-Center points out that toward the close of the 19th and at the opening of the present century the psychology of Wundt and his students was concerned with the systematic question of mental elements. It is not surprising that early work upon affection was directed toward the problem of mental elements. Typical questions of this period are: 'Is affection an element or an attribute of sensation?' 'What qualitative dimensions of affective experience can be demonstrated?' The polemic of Titchener and others against the Wundtian theory is well known. The experiments upon mixed feeling, clearness, localization, extensity and volume are reviewed. Beebe-Center points out that to-day the term "sensation" is not used to refer to a constitutent of experience but rather to a conceptual entity constructed from observed experiences. The same distinction holds in the discussions bearing upon the nature and existence of an affective element. Mental elements are constructed by psychologists with different systematic backgrounds. Observers with such different

backgrounds give varying and to some extent conflicting reports. These reports are then interpreted in ways which yield conflicting conclusions. "The views that hedonic tone is an attribute of sensations, of complexes of sensations, of qualitatively unidimensional affective elements, of qualitatively pluridimensional affective elements, are not inconsistent descriptions of experience, but alternative hypotheses constructed from experience. Again the views that affections may and may not give rise to mixed feelings, are and are not clear, localized, or extended, do not constitute pairs of contradictory characterizations of a single entity, but independent characterizations of different entities (p. 106)." Rather than to embark upon the construction of affective elements whose adequacy must remain problematical, Beebe-Center believes it is wiser to regard the problem of hedonic tone in relation to mental elements as if it did not at present exist.

In a postscript three conclusions are stated "whose interpretation is not at all dependent upon controversial systematic issues." It seems to the reviewer, however, that the postscript commits the author to the systematic position upon which the validity of these propositions depends.

Chapter IV, upon hedonic tone in relation to primary external stimuli, contains 102 of the total 427 pages. There is much detailed discussion of the relation of hedonic tone to sensory quality, to the area and form of stimuli, and to the intensity of stimuli; of experiments involving quantitative measurement; of the Weber-Fechner law for hedonic tone; and of temporal characteristics such as duration, temporal form, position in time, and time error, in relation to hedonic tone.

The reviewer is impressed with the very respectable amount of experimental work already accomplished on the stimulus-conditions of hedonic tone; but also the impression is gained that affective psychology is relatively over balanced on the sensory-stimulus side. The reader of this chapter should perhaps be cautioned that a judgment of liking and disliking is equivocal so far as pleasant and unpleasant experience is concerned.

The problems of affective contrast, adaptation, and habituation are taken up in a separate chapter. The general point of view is that hedonic tone depends not only upon primary stimuli but upon secondary external stimuli which may occur simultaneously or successively.

An interesting and important chapter considers hedonic tone in relation to motivation. Our likes and dislikes are determined by cognitive and evaluative dispositions. They are also determined by physical needs such as the need for water, for food including such specific substances as salt, sugar, and acid. The facts indicate that hedonic tone is markedly influenced by physiological needs; the persistence of a need is accompanied by U and its removal by P. The relationship between hedonic tone and need, however, is an indirect one. The link between the two is probably the stimulation of internal sense organs. Psychological motivating factors, such as wishes, will ultimately be ruled out of scientific psychology, Beebe-Center believes, but they are considered. The fulfillment of a wish usually determines P and thwarting determines U, the degree of P or U varying with the strength of the wish and the strength of its resistance.

Changes in hedonic tone undoubtedly take place with development but it is difficult to know how far learning and how far maturation is involved. In the infant emotional reactions alternate without appreciable transition. These changes are largely determined by physiological states such as hunger. In early childhood hedonic tone can

be reliably inferred from emotional behavior. With the child of 3-4 yr. old, wishes, usually for trains of activity such as playing with blocks, determine hedonic tone. Developmental changes in relative preference are demonstrated for color (Marsden, Shinn, McDougall, Valentine, Dashiell, Winch, Garth, Michaels, Dorcus); for musical intervals (Valentine, Dashiell); and for food stuffs (Davis). The experiments of H. E. and M. C. Jones, and those of Watson and Rayner, upon conditioned emotional reactions are described in the context of affective and emotional development.

The relation between hedonic tone and muscular and glandular responses is next considered. The responses are reviewed under two main headings: (1) vegetative processes, including those considered in the earlier researches upon the "expression" of feeling, and the galvanic reflex, and (2) locomotor processes. Under the second heading is a valuable discussion of the energy of muscular response in relation to P and U, and a consideration of different types of response, *e.g.* extension-flexion, tension-relaxation, seeking-avoiding.

In Chapter IX different aspects of associative memory are taken up in their relation to P and U. Pertinent experimental studies relative to recall, recognition, forgetting, and learning of P and U materials are reviewed. There is little doubt that learning accompanied by P is generally more effective than learning accompanied by U. Experiments are considered under two headings: (1) memory in relation to hedonic tone *during* learning, and (2) memory in relation to hedonic tone *following* learning. Under the latter heading the author discusses the law of effect. The animal experiments upon reward and punishment do not relate directly to the problem of hedonic tone following learning. The animal work, however, is not incompatible with the view that *as a rule* learning followed by P is more effective than learning followed by U. This view is confirmed directly by the experiments of Tait, Frank, and Ludvigh.

The reviewer gains the impression that mere concomitance of P and U with the difference phases of associative memory has been studied, and that a more dynamic and analytical point of view is needed.

A chapter is given to the neural processes associated with P and U. The work of Bechterew, Goltz, Cannon and Britton, and Bard, indicates that the neural centers for behavior expressive of hedonic tone are in the thalamus or hypothalamus, closely related to the emotional centers. The work of Head and Holmes on patients suffering from thalamic lesions and experiencing affective disturbances is direct evidence of the thalamic hypothesis. Beebe-Center believes that hedonic tone depends directly upon neural processes in the thalamus, even though this contradicts the doctrine that conscious processes depend upon the cerebral cortex alone.

The final chapter is a thorough summary of the theories of hedonic tone. One group of theories stresses the nature of the conscious carrier. Under this heading are the views of Ziehen, Stumpf, Titchener, Warren, Nafe, and Krueger. The mental elements are not found but rather made by the psychologists. Another group of theories stresses the immediate conditions of hedonic tone. Causation is sought in the psychical realm (Herbert, McDougall) or in the physiological realm. The physiological theories are of two types: (1) those which relate hedonic tone to processes specific in nature but not in localization (Lehmann, Marshall and Thorndike, Troland, Herrick); and (2) those which relate hedonic tone to processes in a specific part of the nervous system (Allport, Marston). Allport's view is definite but contrary to fact. The other physiological theories lack verifiable implications however true they may turn out

to be in the long run. What is needed is the formulation of limited working hypotheses capable of experimental verification. One such hypothesis is proposed by the author; namely, that hedonic tone depends upon a specific type of sense-organ process in the proprioceptive group which, under sensory instruction, mediates bright and dull pressure, and under hedonic instruction gives rise to relative or absolute hedonic tone. Hedonic tone depends upon the relative "density" of the process.

In general, the book is a thoroughly competent piece of work. One of its strong points is the reliance placed upon experimental method and result, and the relative freedom from dogmatic statement. Beebe-Center has covered a large, complicated, and heterogeneous mass of experimental material and has shown balance and good judgment in evaluating it. The organization of the book is well worked out. For a long time Lehmann's *Hauptgesetze* was the main reference upon affective processes, and Titchener's chapters in *Feeling and Attention* are still classical sources—but much water has run through the psychological mill in recent years. To-day Beebe-Center's book is the best introduction to the psychology of pleasantness and unpleasantness.

University of Illinois PAUL THOMAS YOUNG

Vision and Colour Vision. By R. A. HOUSTOUN. London, Longmans, Green & Co., 1932. Pp. vii, 238, with 102 figures and diagrams.

This compact volume, along with its predecessors, the *Treatise on Light,* and the more popular *Light and Colour,* leaves the laboratory psychologist tremendously in the author's debt. The topics treated range from units, discrimination of intensity, acuity of vision, visibility of the spectrum, laws of color mixing, hue discrimination data, recurrent vision and flicker phenomena, fatigue and after-images, simultaneous contrast and spatial induction, to theories of color vision and color blindness, color vision and normal variation, a detailed examination of color-blind subjects, and the theory of color vision. To each a separate chapter is devoted.

More adequate justice than is usual in German texts is accorded the American and British investigator. Reeves' table of pupillary changes is adopted, Adrian's and Granit's demonstrations of lateral conduction in the retina utilized, Ives', Jones', and Coblentz and Emerson's data on visibility, hue discrimination and the like, made use of. The researches of McDougall and of Hecht are also freely cited, though their conclusions are not invariably conceded.

Original investigations carried on at Glasgow by the author, over a period of twenty years, set the keynote of the discussion. By applying the methods of statistics, and plotting the data of König, Ives, and others in unusual ways (using reciprocals and logarithms), a number of novel positions are arrived at. Weber's law vanishes into thin air, as does also the duplicity theory of Von Kries. The sharp differentiation of fovea and periphery is denied; and visual purple accepted as the sole photochemical substance—a view not out of harmony with Edridge-Green's. The position of the absorption band in the rhodopsin molecule, as determined by the free periods of its constituent electrons, is assumed to vary about a mean, as do the thresholds of the rods and cones; yielding a normal curve with its peak in the yellow-green. When intensities are low, however, light acts selectively on the electrons whose period is nearer that of violet; for these are more easily knocked out of the molecule by resonance, free or forced (Lorentz' theory). Hence the Purkinje shift in twilight vision (94 ff.).

Many concepts and postulates of the psychologist are utilized to advantage in the

discussion. The distinction between psychological and physical units—often overlooked—is basic. The book leads off with an exploratory attempt to factor given brightnesses into subjective units. Incidentally, Ives' metre-candle-millimetre is given preference over the more theoretical millilambert and photon advocated by Troland (both larger units). Yellow, one notes with satisfaction, is accepted as a primary, coördinate with red, green and blue; though the author is justly suspicious of the criterion of unanalyzability, still flaunted by followers of Ladd-Franklin, to the discredit of psychology as science. Would it not be better to admit the absurdity, *per se*, of any *psychological* primary?

Especially refreshing is the treatment of color-blindness in this text. The psychologist irked by the perennial spectacle of the physicist's warping of the facts of normal and deficient color vision into trichromatic framework heaves a sigh of relief at the frank admission that the Young-Helmholtz theory is both contrary to commonsense and mathematically untenable (223 ff.). Incidentally, Houstoun is one of the few present-day theorists to take cognizance of Helmholtz' altered position on color-blindness in the second edition of the *Physiologische Optik*.

Picking up a suggestion tossed off somewhat sceptically by Rayleigh, in 1881, to the effect that anomalies of color vision may represent merely the extremes of normal variation, Houstoun (in 1918) set out to test unselected populations. Groups of 100, 527, 604, 1148, were rated for hue discrimination, the Rayleigh equation, insensibility to red light (Ch. XII). The results demonstrate the fallacy of positing protanopes (red-blind), deuteranopes (green-blind), and red and green anomalies as distinct groupings. Color-blind, anomalies, and normal visioned are intermingled within the limits of the normal curve, or among the 'outliers.' The classifications utilized so glibly by Von Kries and Nagel are obvious artifacts, constructed on false postulates. As for the Gaussian curve for so-called 'anomalies,' (the group set off by Rayleigh from the color-blind, marked by insistence on a preponderance of either lithium red or thallium green in a mixture to match sodium yellow, and further exploited in 1890 and 1907-9 by Schuster and by Guttmann), half of its scatter proves to be a matter of observational error.

Unique among physicists today, Houstoun takes into account the minute structure of the retina—as well as the electron and quantum concepts—in his theory of color vision (Ch. XIV). Rods and cones are not differentiated as to color functions; but cross-connections and the segregation of synapses in two distinct reticular layers are utilized to explain the peculiarities of dark adaptation, the photochromatic interval, the 'opponent' pairs (red-green, blue-yellow) of the Hering theory.

One could wish, it is true, that Houstoun had assimilated the psychologist's abstraction of the dimension of saturation from that of brightness (or brilliance) more adequately. Like the majority of physicists, working with transmitted rather than reflected light, he apparently lacks a working concept of gray, *i.e.* a middle neutral point of departure for the saturation scale. In the opinion of the reviewer, the inference based on his final crucial 'contouring' experiment that the vision of the color-blind is *trichromatic*, not dichromatic, is vitiated by confusion of saturation differences with those of hue. The color-blind are as quick to individualize *saturation* as brightness differences of yellow, and the former are not ruled out in the procedure. The use of filters (so-called 'monochromatic'—Wratton-Wainwright) instead of spectroscopic light, and of a daylight source, diminishes further one's confidence in this final test.

These are mere flies in the ointment, however, and detract not at all from the value and brilliancy of the book as a whole. It should be on the shelf of every psychologist who wishes to keep abreast of visual research, in touch with relevant findings in nerve physiology and physics as well. The format of the volume is on the usual high level of excellence of the publisher. There is a brief but useful index, chiefly of names.

Cornell University ELSIE MURRAY

Principles of Mental Development: A Textbook in Educational Psychology. By R. H. WHEELER and F. T. PERKINS. New York, T. Y. Crowell, 1932. Pp. xxvii, 529.

Koffka and Ogden, to whom this book is dedicated, long ago convinced us that *Gestalt* had a message for education, could one but understand it. In the ten years or more since we began to take note of these prophets of a new day, *Gestalt* has developed and expanded. It is with high hopes, therefore, that one approaches a book written in large part by one of the converts in that development. These hopes grew brighter upon casual skimming but dimmed with more careful reading. The authors have missed an opportunity. In this review therefore I shall not attempt to assess the *Gestalt* position or its possible contribution to education. Rather it is my ungrateful task to show how Wheeler and Perkins have failed to make that contribution. This involves a selection of detailed points for critical analysis; an honest effort has been made to select weaknesses which are typical.

But first a general statement as to the nature of the book. It is an attempt to write a textbook of educational psychology in terms of the configurational principles. The usual divisions thus receive a reinterpretation and restatement though the classical outline is fairly apparent. One has the impression that topics receive emphasis rather more in proportion as they yield opportunity to expound the Gestalt position than because they are really contributory to the development of teaching effectiveness. None the less there is an obvious effort to show the practical implications of the theory—on the whole pretty successfully. Now for the criticisms.

In the first place, the book is hastily and carelessly written. For what else are we to make of such circularity of statement (if not of reasoning) as occurs on page 5? "Any [such] point of view . . . will yield fatal results, because the point of view is wrong and leads to incorrect methods." This is, unfortunately, typical of the loose logic and language throughout. Thus: "It has been discovered that behavior follows the laws of dynamics; there is, therefore, no 'original' *vs.* acquired nature for the modes of response indicated by the terms instinct and learning follow the same laws" (p. 122). This, I submit, is a non-sequitur. Clearly it is possible for the two modes of response to follow the laws of dynamics yet be differentiated from each other. In technical terms our authors incorrectly imply the distribution of the predicate.

Formal logic is somewhat less clearly violated, perhaps, in the discussion of learning to walk. "Awkwardness . . . is applied to one movement when it is compared with another, made at a later stage in the learning process. But the earlier and later movements are made under *different* sets of conditions, hence it is illogical to compare them." Yet we find the authors two pages later comparing finer coördinations with grosser and less differentiated ones—which are surely "made under different sets of conditions." I concede that the authors have a point, comparisons must take account of differences in conditions, but the point does not seem to have been clearly thought out and we therefore get a blanket condemnation of a perfectly good

concept like "awkwardness," despite the fact that their own use of "skill" implies its opposite. Much of this carelessness of statement goes back, it seems to me, to an all pervasive dogmatism which leads to the too easy solution of problems by purely verbal devices. Again and again, once a phenomenon is "explained" as being a manifestation of a total pattern formed under dynamical laws, the problem is left there—half solved. For example, the correct timing of a skilled movement, the authors contend, cannot be attained by putting together separate movements; they must emerge from a total pattern. But why the correct pattern emerges—which is the critical point—is left unexamined, apparently because of the magical virtues of "pattern as a whole" and "dynamic laws."

Our authors clearly dislike the concept of original nature. Hence they refer to Freeman's investigation and interpretation but ignore Burks'. They quote with understandable chortles Thorndike's experimental refutation of Thorndike's law of repetition of a response. They do not mention Thorndike's strictly parallel experiments showing the ineffectiveness of the repetition of a stimulation; their own theory centers around that.

Sometimes, to be sure, the dogmatism seems as much a matter of style as anything else. Wheeler and Perkins flourish "laws" as making a thing necessary or impossible on virtually every page. They are fond of such expressions as "demonstrates" or "proves" and "disproves" where a sounder phrase would be "is evidence for" or "against." I doubt that Peterson would say, as Wheeler and Perkins do for him, that his well-known experiment "disproves the repetition theory." In neither scientific nor affective logic are theories so easily dispossessed. Perhaps it was some faint realization of this which led them to buttress this position by another resounding dictum which I quote entirely: "Moreover, that learning does not take place when the conditions are satisfied for the repetition theory, is also conclusive evidence against the theory" (p. 350). Again one notes the straining for certainty in that "conclusive." For the rest, no one can quarrel with the dictum, which merely restates one of Mill's canons. Surely, however, the issue is whether the conditions were in fact satisfied. That issue is here evaded by the vigorous assertion of a platitude. The climax of dogmatism is reached with a summary of "facts about the learning process" (p. 275 ff.). Nos. 5 and 12 may be quoted to show the character of these "facts": (5) "Learning is the expression of a potential." (12) "Each step in the learning process is organized under the law of least action."

These are interesting and legitimate speculations. I do not object to their being presented even to elementary students as a statement of the authors' credo. But to dub them facts is to give the student an altogether false impression of the situation.

Enough has been said to indicate, I think, that this is a thoroughly dangerous book to put in the hands of any uncritical reader; and more's the pity, for it has some admirable features. The attitude towards education is enlightened and progressive, though not always closely connected with the psychology. But, at any rate, a number of significant psychological principles are focussed in support of a more enlightened scheme of education. Furthermore, a variety of experimental evidence is skillfully marshalled in support of a theory of learning or at least of an orienting point of view which is interesting and significant.

Credit also must be given the authors for the tremendous task of restatement of familiar facts in the configurational language. It is no slight task to tear so many experimental facts loose from their accustomed interpretations and show, concretely,

that they fit in the patterns of thought of the new theory. The task is, moreover, important; a theory is to be judged as much for its ability to assimilate established fact as for encouragement of *ad hoc* experimentation. But to put the matter baldly, the skillfully marshalled data and the significant schemata under which they are ordered would emerge more clearly and would be more impressive if there were less tub-thumping.

Ohio State University HORACE B. ENGLISH

Methoden zur psychologischen Untersuchung der Wirbeltiere. By WERNER FISCHEL. *Methoden zur psychologischen Untersuchung der Wirbelloser Tiere.* By F. J. J. BUYTENDIJK and WERNER FISCHEL. Handbuch der biologischen Arbeitsmethoden. Edited by Emil Abderhalden. Abt. VI. Methoden der experimentellen Psychologie, Teil D. Heft 3. Berlin, Urban & Schwarzenberg, 1932. Pp. 233-338; 339-419.

From the title of the first study one might expect it to be a critical treatment and evaluation of methods in animal psychology, but it is not. Methods are discussed to the same extent that they are in any critical treatment of animal psychology. On the whole the discussion is not critical, and at points where the author is critical it has to do with the interpretation of results rather than method. As a review of animal psychology the study is very well done. The problems are carefully stated and in most cases the studies referred to are accurately presented. No background in animal psychology is required of the reader.

In the introduction Fischel points out some of the difficulties met with in animal experimentation: The more important of which are: (1) the difficulty in knowing when an animal is performing at its maximum capacity; (2) the difficulty, in studies of intelligence, in establishing the relation between experience and insight; and (3) the difficulty, if an animal has higher abilities, in excluding these in order to study the more simple.

The first of these suggests that motivation and attention must be considered, yet studies dealing with these topics are omitted. The second and third points are used to some extent in the evaluation of the experimental results.

The scope of the article includes perception, learning, and insight. Under perception the author reviews typical experiments, with various animals, on the discrimination of brightness, form, color, depth, and the response to common elements; and the response to relationships having to do with relative size, brightness, and quantity. He wisely argues against the interpretation that the response to a relationship is a demonstration of insight. Due to the fact that various methods in discrimination experiments give varying results, a discussion of the dependence of results on the measuring instruments would have been highly desirable.

Under learning, experiments on enclosed and elevated mazes, orientation, delayed reaction, understanding, the breaking up of habits, and memory (in so far as it concerns what might be called *expectation*) are discussed. The treatment of maze learning is largely limited to a comparison of the two types of mazes, the problem of kinaesthesis, and a discussion of the use of the maze for comparing normal and variously treated experimental groups of rats. There is no attempt at an analysis of learning. Numerous experiments on delayed reaction are cited, but no attempt is made to evaluate or to harmonize the varying results obtained from different methods.

Hunter's "sensory thought" is regarded, not as an explanation of delayed reaction, but as the very thing which has to be explained. The function of the goal in learning experiments cannot be merely regarded as that with which a certain act is associated, but rather it must be regarded as an end which the animal desires to arrive at and satisfy a need. Experiments by Tinklepaugh and the author are presented to show that animals respond to specific memories of specific rewards.

Insight is defined as an ability to foresee the consequences of an act. To demonstrate this ability (1) a difficulty must be overcome without trial-and-error; (2) chance solutions must be excluded; and (3) the problem must not be simplified by demanding behavior which is natural for the animal.

These specifications are not based on any particular analysis of insight and appear to be merely an attempt to exclude the personal factor. The author is very critical of the influence of past experience in many of the tests of intelligence. He seems to insist that in order to credit an animal with insight it must do something new, but he does not tell us the qualifications of a really new act. He states that primary solutions have only been achieved by animals at the level of monkeys. Maier's experiments on rats which are not excluded by any of the above specifications are inaccurately presented as experiments on transfer. With this error in statement these experiments naturally fall under secondary rather than primary solutions.

The second monograph is a review of experiments made on invertebrates (excepting bees and ants) with special emphasis given to method. It is principally an analysis of learning in various animal forms.

The most simple phenomenon akin to learning is that of adaptation (Gewöhnung). This phenomenon cannot be a matter of fatigue. The animal either ceases to react or modifies its reaction because of previous stimulation. The apparent learning of Paramecia (capillary tube experiments) is shown to be due to changes in the constitution of the water. The behavior of both Protozoa and Coelenterata are found to depend upon immediate stimulation. The past affects these forms merely by producing adaptation.

Echinodermata, however, are capable of true modification by experience. When star-fish are fastened to a platform with wire they succeed in escaping by random activity. After several trials they concentrate on the successful line of activity of certain arms. Out of their repertory of reactions one pattern finally dominates because of past experience.

Among Annelides the authors find cases of association which are demonstrated by learning of the T-maze. These animals are regarded as forming a habit by repeating previous responses, but the function of memory is limited to the reflex level.

Mollusca show adaptation which transfers from one stimulus situation to another. (This reminds one of the spread of inhibition in conditioning experiments.) There are also experiments with these forms which show true conditioned responses as well as the learning of Y-mazes. These forms, however, are at best only capable of forming position habits and cannot learn to respond to a characteristic difference between stimuli.

Learning in lower forms differs from that in higher forms in that the reward determines the behavior in the latter, whereas the lower forms respond by repeating previous acts. Thus lower forms respond to immediate stimulation plus the associations connected with the stimuli. The future is non-functional in determining behavior.

Crustacea are superior to the previous forms in that when trained to go to one side of a Y-maze they show a reduction in time when trained to go to the opposite side. Because of this ability the authors regard them as the lowest forms in which memory definitely modifies later behavior.

The discussion of Arachnidae and Insecta emphasizes instinctive behavior and its relation to past experience. No new criteria of learning are presented, but it is pointed out that among insects there is learning in discrimination experiments. Instinctive responses are regarded as dependir g both upon internal and external stimulation. Reflex and chain reflex theories of instinct are criticized in the light of experimental results.

From the analysis of learning presented one may make out several levels of learning. These are (1) adaptation, which is a modification of behavior by experience, but not true association; (2) the elimination of certain responses, but not the formation of new ones; (3) the formation of associations at a reflex level; (4) the formation of a position habit; (5) the formation of responses to certain characteristics in the stimuli; and (6) the learning of responses in which memory functions so as to make the response dependent upon future consequences. The latter is true learning.

These distinctions, it appears to the reviewer, are purely artificial and are seemingly determined by the authors' supposition that lower evolutionary forms must be simpler than higher forms. This supposition may be true, but how adaptation and the elimination of certain responses by past experience differ is most unclear. Nor is it clear in what way the position habit differs from the elimination of all but one of several responses; or why the position habit is not a response to certain sensory (kinaesthetic) differences. If a position habit furnished no sensory differences how would it be possible to set up differential responses? The most unsatisfactory distinction is that between mere association and learning in which the future plays a rôle. A dog is said to go to food in a learning situation because of food, but Mollusca do the same thing by repeating a previous response. The evidence for this distinction is that lower forms require as many trials to learn to reverse a position habit as they do to learn the original habit; whereas higher forms show a saving or transfer.

Despite these objections we must regard this study as stimulating and carefully worked out.

University of Michigan NORMAN R. F. MAIER

Experimental Psychology. By JOHANNES LINDWORSKY. Translated from the fourth German edition by Harry R. DeSilva. Preface by translator. New York, Macmillan Co., 1931. p. xix, 406.

In this book psychology is the science whose subject matter is the world of consciousness. Its method is introspection and its problem is to describe its subject matter, classify its facts, and explain them. Explanation may be in terms of antecedents and consequents within the realm of the psychological when facts are complete or it may be in terms of the physiological origin of psychological events. The difficulties of privacy of conscious content which led to objective psychology are averted by the device that psychology may include within its investigations expressions of conscious states, chiefly through language. By this means one may know the nature of the conscious states of another by highly reliable analogy.

Sensation is attained by abstraction, results when the sense organ is stimulated in the simplest manner possible, and is defined as "a relatively independent, simple, concrete, palpable, objective conscious content." Act is distinguished from content in sensation but the author does not carry the distinction of act and content systematically through his psychology. "Sensation complexes" are produced when a multiplicity of stimuli simultaneously strike a single sense organ or more than one sense organ. Such complexes sorely need investigation and will be shown to be of great importance systematically. Grouped under sensation complexes are, first, tonal fusion because it is the simplest case and then visual and tactual space perception because they possess the attribute of extension denied auditory experience. Image though not attributively distinguishable from sensation is, nevertheless, classed with sensation as a psychical element.

The next psychological construction is the combination of images with sensations and sensation-complexes. This is a field so little touched by experiment that the author, confronted by dearth of facts, gives a few examples of these constructions but can go no further. Synaesthesis is one of the examples and it exhibits a characteristic of all, that completory images constitute part of all perception.

Returning to the elementary we find that cognitive content though not unequivocally granted the status of element commensurate with sensation and image because it is impalpable is, nevertheless, basic and ranks as "a genuine conscious content" which has been shown experimentally to occupy the "mid-point" of consciousness. "Knowledge of relation," "active relating," and "abstraction" are the examples of simplest cognitive achievements. Advancing to the complex once more there is given a group of complex structures, the "combination of the function of relation with sensation and sensation-complexes." Through the creation of psychological structures of this order the restrictions of a psychology of sensory elements are abrogated and the way is open to incorporate *Gestalt* perception and perception of movement and time into the author's system of psychology.

Two more elementary experiences complete the list of the elementary: feeling and volition. Feelings are "real experiences" and may not, in spite of the systematic difficulties they offer, be omitted from psychology. Although the facts of feeling are competently presented, feeling as an entity stands rather aloof from the systematic psychology of the book as a whole. In the matter of volition, experimental evidence justifies the conclusion that there is a characteristic experience of volition. Its peculiar property is that it is "an inner activity" and it must be directed toward a goal and is, consequently, able to reveal itself only through a "mode of behavior."

In this review it is not necessary to present in detail the author's treatment of what is conventionally called memory and association. Aside from his individual terminology the essential unique feature is his emphasis upon the fact that in mental life association is a constellation. One image is never given alone in consciousness but is always associatively connected, through the experiences of life, with a network of other images by associative connections of unequal strength in which, in recall, processes of inhibition and facilitation operate, determining what images will appear in consciousness.

We now pass to the "higher mental achievements of adults": "higher cognitive achievements," "higher feelings," and "the volitional life." Throughout the range of higher cognitive achievements which are complicated structures of psychological units a mode (or modes) of awareness of relation is an indispensable constituent.

These structures are comparison, apprehension of objects, the practical discrimination between perception and image, formation of concepts, and the establishment of certainty. Inferential thinking is the apprehension of known relations. In the forms of productive thinking where voluntary activity is essential there are also present relational elements. Other terms in the psychology of thought need not be mentioned specifically. Memorial experiences are also components of these higher cognitive processes but especial emphasis is laid upon relational constituents. Even in the higher achievements of memory themselves emphasis upon relational activity is maintained. In the absence of adequate facts in the field the author finds himself unable to complete a doctrine of feeling. The measure of a higher feeling is that in its production "the knowledge of a material relation conditions the reproduction of a complex feeling." Volitional life is dependent upon motives. A motive is "everything which is represented to the mind as a value realizable through the voluntary act," and value is anything that is advantageous for the individual concerned." When the resolve to act has been made and, as in the reaction experiment, the stimulus to act is given there results a determining tendency as the effect of the act of volition, which carries the action to completion. Although the author discusses the rôle of the image of the goal at some length it need not be reviewed here. Reflexive, instinctive, and volitional action are called "outer" actions in contrast with "inner volitional action" which is the result of an act of volition and is important for what follows. Not every phenomenon of attention is classifiable as an inner volitional act but the baffling status of attention in psychology tempts the author to make it, in part, such a volitional act. This position is made plausible on account of the fact that movements of the sense organs are mechanisms of attention and that traditionally attention is "a certain voluntary or involuntary behavior of the individual." It follows curiously but naturally that if attention is an inner volitional action then images "move." "Controlled movement" of images is the "guidance of thought processes by a task;" and fantasy, "free movement of images." This free movement reveals talent, is closely related to productive thinking, and requires the largely fortuitous development of a new apprehension of relation in the complex to establish it as a creative act.

The book closes with a treatment of socially influenced mental achievements: language, morals, art, and religion, and with the exceptional mental states: sleep, dreams, and hypnosis.

The critical features, its full presentation of fact, its suggestions for further research, and its highly individual organization combine to make it an important contribution to psychology.

Bethel, Maine H. G. BISHOP

Experimental Psychology. By H. GRUENDER. Milwaukee, Bruce Publishing Co., 1932. Pp. xi, 455.

In spite of the growth of "experimental" psychology beyond the limits of any particular set of interests or tenets of any school, the term suggests to most psychologists facts and theories derived from laboratory sources. Consequently as one approaches a book with this title he is apt to have a more or less definite "mental" set as to what he shall find in it. Even if the book does not contain an abundance of statistics, tables, graphs, and illustrations, as some of the best treatises on the subject do not (viz., Fröbes and Myers), nevertheless one expects the author to stick closely

to factual material, to eschew argument as far as possible, and to treat theories and points of view differing from his own with the fairness and tolerance characteristic of scientific objectivity. But the general tone of this book is not that of the experimentalist in several important respects, chiefly in its doctrinate views and its use of innuendo and summary dismissal of facts which do not fit into the author's system. It is not often in a book on experimental psychology that one finds Aristotle's and the schoolmen's views treated with more respect and consideration than the findings of experimentalists; *a priori* definitions made the touchstone for the acceptance and rejectance of facts; theories involving the ego, free will, and "an All-wise creator" accepted while Titchener's theory of meaning is dubbed a "metaphysical darkroom." Yet certain parts of the book display keen critical insight and old material is presented with a vigor and freshness that make interesting reading even for the psychologist familiar with it all. A brief discussion of certain topics will show more clearly why this book impressed the reviewer as it did.

Gruender's definition of psychology as the science of conscious life, concerned with "the facts of internal experience" which are private but have a subject-object character, reminds one of Brentano's psychology but for its insistence upon a thinking subject or ego. While animal behavior strictly cannot be included in psychology if we accept this definition, it is included on the basis of its analogies with human behavior. But it remains only an interesting set of analogies because there is an impassable gulf between human and animal behavior. In fact we are told that "we had better keep the monkey out of the psychological laboratory. He is really out of place there" (p. 128). Köhler's experiments "have done nothing to further the solution of the problem of animal intelligence" but the author's sensitive-impulse theory of instinct does, even though it "supposes the existence of an All-wise creator Who directs all His creatures . . . and safeguards man's unique place in nature as king of the visible creation" (p. 300).

There are several excellent chapters on vision, hearing, thinking, and attention, the latter being full of practical suggestions. But sensations of hunger, thirst, nausea, dizziness, pins and needles, itching, tingling, and "others of a rather obscure nature are better left to the physiologists for detailed discussion," although the reasons for abandoning them so hurriedly are not given. The work of the Würzburg school receives able defence and Titchener's position on this subject is successfully assailed, in the reviewer's opinion. The discussion of the various theories of thinking fills a lacuna in psychological texts which usually neglect this subject entirely.

Throughout the book the author struggles valiantly with sensationalistic psychology and atomistic physiology, without making clear to this reader where the line is to be drawn in our acceptance of these principles. Thus sensations are accepted as the nucleus of mental processes "stripped of all accretions" but a sensationalistic account of all thinking and the will is rejected. Likewise the neural groove theory with its assumption that repetitions strengthen associations is accepted as adequate for sensory phenomena but not for intellectual processes. We are thus in the presence of duality throughout the gamut of psychological processes: perceptions may be analyzed into sensation but the ego resists analysis; some thinking is merely reassociation but recognition requires association plus [*sic*] the ego; some actions are purely automatic, others subject to voluntary control or "free-will."

The author's command of modern movements in psychology may be gleaned from his placement of sensationalism with *Gestalt* psychology: "sensationalism . . . is

really one of the many varieties of the theory of *Gestalt;* namely, that variety which admits no units of structure except sensations" (p. 19). In the foreword by the author we are told that "this book is the outgrowth of a series of lectures given to graduate students at the St. Louis University."

Bryn Mawr College HARRY HELSON

Maternal Behavior in the Rat. By BERTOLD P. WIESNER and NORA M. SHEARD. Edinburgh, Oliver & Boyd, 1933. Pp. xi, 245.

This monograph describes and analyzes maternal behavior and its physiological concomitants in the albino rat. In point of view, the authors display a persistent leaning toward dynamic formulations and in so doing place the major emphasis upon endogenous rather than exogenous causes of specific behavior mechanisms. At the same time they do not fail to take account of external stimuli and field situations; it is merely that they elevate the intraorganic factors to new ranks of importance in the total array of causative agencies.

According to their analysis, maternal behavior is comprised of several relatively independent components; namely, parturition, nest-building, retrieving, defense, licking of young, and nursing. All of these components are sufficiently discrete to admit of clear-cut definition, yet some, notably retrieving, serve better than others as indicators of maternal drive in a complex series of investigations.

The major problems studied are as follows: prolongation of maternal behavior beyond the usual times; the incentive value of young rats of different ages; maternal behavior toward the young of alien species; retrieving, by virgins put into a variety of maternal situations; the rôle of parturition as an instigator of maternal responses; and finally the effects of destruction of the mammillae, removal of the ovaries, and injections of endocrine extracts.

Illustrative of their findings are the following facts. Primiparae and multiparae which have ceased to take notice of their young, when the latter are moving freely about the cage, will foster the very young rats of other mothers for a long period of time if the young are made accessible. Two phases of prolonged maternal behavior appear: (1) an initial phase, ending about 6 weeks after parturition, in which almost all mothers will display vigorous maternal behavior toward foster young in the helpless age, their behavior being scarcely distinguishable from that during the first two weeks after parturition; and (2) a terminal phase in which two groups are to be distinguished, in one of which maternal behavior wanes some 2½–6 mo. after parturition as shown by component after component dropping out of the total maternal ensemble, but in the other phase vigorous maternal responses continue for 14 mo. or more. In the early post-partum period subsidence of nesting seems to be less a function of time than of the availability of materials and the age of the young, but time is not unimportant as eventually all nesting ceases even under the most propitious field conditions. Nursing postures of the mother usually disappear with lactation, but sometimes they are exhibited after total cessation of milk secretion. Maternal drive will appear and be maintained in parturient females in the absence of turgor of the mammary glands and even if the latter have been completely excised; likewise, in the absence of the ovaries, post-partum maternal responses appear and continue until the end of the normal suckling period.

By supplying young mice, rabbits, and kittens to parturient females it was found that the incentive value of such alien species for the induction of maternal behavior

is greatest when the young are in the helpless stages of immaturity. Helplessness, rather than size or weight, appears to determine the mothers' attitude.

Using retrieving as the principal indicator of maternal drive, the authors found by extensive tests that some virgins, when isolated in the cages employed in the maternity ward, will carry in (retrieve) very young rats; others, not reacting in initial tests, will do so if given young rats as nest-fellows for a few days; still others fail to retrieve in either situation. The latter were especially useful in crucial tests on induction of retrieving through the injection of various gonadotropic extracts, particularly those from the anterior pituitary body or from pregnancy urine. The authors demonstrated that retrieving can be induced by these gonadotropic extracts but that control injections of extracts from other tissues did not induce similar responses. So far, little is known as to the specific component of the extract that induced retrieving, but no doubt further work with simpler fractions of these extracts will clarify this point.

The absence or weakness of other components of maternal behavior when retrieving was induced by the injection experiments left one in a quandary as to whether retrieving, as induced by gonadotropic injections, was essentially the same as retrieving appearing with motherhood. In appearance they are alike, but of course their internal motivation may be quite dissimilar. Future analyses may throw some light upon this important question, for, as the authors state, their present investigations do not purport to solve all problems to which their efforts have been directed.

Throughout the book the authors have displayed a nice balance between exposition, critical evaluation, and correlation of their own findings as well as those of others. This kind of monograph will have high interest value for students and teachers alike in the field of psychobiology.

Stanford University CALVIN P. STONE

Growth and Development of the Child: II. Anatomy and Physiology. By HALLOWELL DAVIS and others. The White House Conference on Child Health and Protection. Report of the Subcommittee on Anatomy and Physiology. New York, Century Co., 1933. Pp. xxi, 629.

A number of sections of this volume deal with topics of interest to physiological and genetic psychologists. Among such significant sections are those which deal with the following topics: the early stages of the embryo, the nature of voluntary musculature, growth of the skeleton development of the face and dentition, the central nervous system, the development of reflex behavior, the development of centers in the spinal cord and brain, the visual and auditory mechanism, and the general bodily functions in childhood, including a rather detailed consideration of the glands of internal secretion. Most of the sections are followed by brief and generally well chosen lists of references. The distinction of the editors, the special committee, and the able list of contributors make this volume a significant brief summary of facts important for all those who are interested in the normal development and the pathology of children.

In this brief review no effort will be made to indicate the many admirable characteristics of the book. Instead, and possibly ungraciously, some indication will be given of certain statements which seem to be so popularized that they present a misleading impression of modern knowledge in some of the fields dealt with in the book which touch upon scientific psychology.

In a consideration of the development of function in the nervous system, for example, much emphasis is given to the theory of myelinization without sufficient treatment of the objections to this theory. The account of the early development of reflex behavior is so brief as to be scarcely adequate to this important subject. It is interesting to note, moreover, that on pages 195 and 196 the work of Minkowski, Bersot, and Bolaffio and Artom on fetal reflexes is briefly reviewed and then on page 213 the surprising statement is made that "Minkowski is the only investigator who has studied the movements and type of reflexes found in young human fetuses." Only five pages are devoted to the nerve "centers." These pages are possibly satisfactory enough in what they do present, but it is unfortunate that in the list of references given, as well as in the treatment presented, Lashley's work is not reviewed, although the earlier work of Franz is considered.

The treatment of the visual mechanism is opened with the pessimistic statement that "an understanding of the visual process requires a knowledge of three factors: (1) the nature of light, a problem in physics; (2) the conversion of light into a nerve impulse at the retina, a puzzle in physiology; (3) the conscious response to this impulse, a perplexity in psychology." In this volume the problems, puzzles, and perplexities of this subject are not made less by the teleological language in which its treatment is couched, as may be shown in the following example: "Nature tries to offset these errors through a small pupillary diaphragm, and through the catenary curvature of the cornea and lens." The consideration of visual depth perception is so open to criticism that it may well be quoted here in full:

"Light, color, and form perception are possible to either eye singly. Judgment of depth may be crudely accomplished by one eye through accommodation consciousness, through the interpretation of the size of the retinal image in terms of the known size of the object giving the image.

"Spontaneous and accurate visual perception of depth requires the coördinated use of the two eyes affording two images of the object viewed from different angles, and the building up of these two images in the brain into a concept possessing three dimensions, length, breadth, and thickness. The ability of the brain to correlate two such images is commonly called the fusion faculty. The fusion faculty is not present at birth, but is developed as the visual acuity grows sharper, as the ocular muscles become coördinated, and as the brain learns to fuse the images during the first few years of life. The fusion faculty develops imperfectly, or not at all in those individuals in whom there is a lack of one or more of the essential prerequisites."

The treatment of the ear is largely anatomical. The fact that Dr. Hallowell Davis, eminent as an experimental physiologist in the field of audition, is chief editor of the volume makes such statements as are given in regard to auditory function seem quite unexpected. The following quotation will make this clear: "The internal ear, or labyrinth, simply takes up the sensations which are conveyed to it through the air or other media, and pass [passes] these sensations to the brain for elaboration and appreciation."

The treatment of the internal secretions is more complete than most of the other sections in the volume. The final section of the work, "Increments of Growth," is largely based upon the excellent work of Jackson, Paterson, and Scammon.

Evaluation of this volume is difficult. The importance of the book has been pointed out. It must be admitted, however, that it can scarcely be considered as a textbook and still less as a handbook or as a contribution to knowledge. Rather the

work must be thought of as a very unevenly valuable summary of some of the main facts which bear upon early human development. For some workers with children it will have a great value. It seems, nevertheless, in many ways to be a duplication of material already available, often in more complete form, for those who have access to a few of the standard works in this field. It is especially to be regretted that some statements that are not fully accurate have been allowed to creep into a volume which will by some be considered naïvely to possess the authorization of the Federal Government.

Brown University LEONARD CARMICHAEL

Psychology Today: Lectures and Study Manual. Edited by WALTER V. BINGHAM. Preface by Levering Tyson. Chicago, Univ. Chicago Press, 1932. Pp. xii, 271, 185.

These radio lectures are divided evenly among general fields of psychological investigations—Psychology Today, Child Development, Our Changing Personalities, Animal Behavior, Psychology of Education, and Psychology and Industry. Five psychologists contribute 15-min. talks in each of these fields. The lectures in the first division are either of an introductory or of a somewhat general nature; those in the other five divisions deal with more specific problems and conquests. There are, in addition to these lectures, which cover 271 pages, 185 pages in the latter part of the book which includes the six *"Listener's Notebook"* materials. These materials likewise cover the six divisions mentioned, but the paging in this section is not continuous through the several divisions, a matter which adds to the difficulties of turning to references by readers who prefer to go to references as they are encountered in the lectures. Every *Listener's Notebook* includes helps in the form of general introductory statements, summaries of lectures, questions, tables, references, pictures of the lecturers, of human and animal subjects, and of apparatus. The lectures are, of course, of a semi-popular sort, but on continuous reading they give the impression of a rather good presentation of the investigations that psychology is carrying on effectively into various fields of human activities, shedding light on processes of work and living which have in the past been much neglected. Many of the lectures are entirely free from the tendencies to moralize (in the form of "should do" this and that) or to make explanations and applications on the basis of inadequate data. They plunge directly into narration of work being done, of problems being investigated experimentally from this and that angle by the trying out of hypothesis after hypothesis until basic relations are revealed. This sort of lecture comes mainly from investigators who play leading rôles in the actual research in their several fields. Usually a fair degree of caution against over-enthusiasm and inaccurate statements is evident. The authors, however, may possibly regret the occurrence of statements like these (italics the reviewer's): "Therefore, at one pause, and while holding still, *they* [the eyes] *recognize* or take in two or three words," etc. (p. 17); "Not only attitudes toward nationalities, but most of the other important attitudes that we cling to intensely, seem to have their *foundations in childhood experience*" (p. 32); "Indeed nature seems to be prolonging human infancy *in order to* make more mental growth possible" (p. 51); "To draw a vertical line is one ability; to draw a horizontal line is another ability. Each ability must await its turn in the order of development" (p. 55); "Children play games involving elements of danger, partly *because of the joy of escape,* and partly *because of this pleasurable agitation*—the tonic result of just a momentary fear" (p. 59); "This [stroking the sensitive skin of the infant's

body and of his mouth] *is the basis of all emotional love responses,* love of mother, father—love between the sexes;" "All true [*sic*] mental disorders have a physical cause" (p. 134); "In the great apes, however, such ideational or *thinking behavior* has been demonstrated experimentally again and again" (p. 140); "The animal then becomes dominated by a single impulse, and searches about for the proper object to satisfy the inner craving. *Nature has seen to it* that the drives which are necessary to survival are deeply rooted in the organism and transmitted by heredity" (p. 154 f.). H. M. Johnson (*Psychol. Rev.,* 39, 1932, 293-323) and Willis D. Ellis (this JOURNAL, 44, 1932, 365) have dealt with other unfortunate errors either of fact or of implication and have stressed the importance of adhering strictly to fact in scientific lectures of this kind. Despite such interpretations and over-positive statements as illustrated above this series of thirty lectures represents a valuable experiment toward putting the layman in more close touch with current research work in psychology. While not all research scientists are successful radio lecturers, the twenty-seven psychologists (two of whom gave two lectures each) and the physiologist (W. B. Cannon) constituting the lecturers heard in these series were all well known and include a number of eminent investigators.

J. P.

The Reading Abilities of College Students: An Experimental Study. By ALVIN C. EURICH. Minneapolis, Univ. Minnesota Press, 1931. Pp. xv, 208.

This investigation "sought to evaluate the efficacy of reading, vocabulary and study exercises in improving reading comprehension, rate of reading, specific vocabulary, general vocabulary, retention in reading, English composition, general scholarship, and general mental ability as measured by the Minnesota College Ability Test." A number of interesting tests were revised or invented, including those for reading rate, comprehension and retention, and specialized vocabulary lists. Drills on paragraph reading, vocabulary from readings, and how-to-study exercises were given to paired sections of freshman English composition students for a total of about 6 hr. during one quarter, with one group running a second quarter. Results showed significant improvement on the specific vocabulary taught but in practically nothing else, and they were confirmed by subsidiary experiments.

In the opinion of the reviewer, this experiment brings together systematically a large amount of information on details of reading improvement, and the measurements need not be repeated by others unless they can show reasonable qualitative grounds for expecting different results. The measurement angles of the study seem almost top-heavy, including large numbers of full page tables and appendices and a continual reference to statistical results in the text. The method of teaching was, perhaps necessarily in their group situations, a "shotgun" attack of teaching the same things to all without regard to diagnosis of their individual difficulties. Because this study dealt primarily with groups of average students, in a non-diagnostic way and for only a short time, it is quite possible that the findings of insignificant progress are still compatible with the much greater progress reported by other investigators for poor readers under more intensive individualized instructions. The situation seems a close parallel to the experiments on transfer of training which show small amounts of transfer under the ordinary type of "practice" but very significant transfer when due attention was paid to the qualitative aspects of training.

Eurich's study strongly reflects the present tendency in educational psychology

to place the major emphasis upon the accuracy of the statistical techniques employed at the expense of the qualitative analysis of the problem. Its main importance is that of providing measures and results for checking certain current beliefs about the improvability of reading and pointing out the inadequacy of some commonly used procedures. It should thus save the time of future workers in the field by showing what not to do, but a thorough survey of the possibilities of reading improvement would necessarily include a great deal more study of the diagnosis of particular needs and the development of the most appropriate methods of dealing with individual cases.

University of Oregon ROBERT H. SEASHORE

Psychopathic Personalities. By EUGEN KAHN. Tr. from Bumke's Handbuch der Geisteskrankheiten by H. Flanders Dunbar. New Haven, Yale Univ. Press, 1931 Pp. xii, 521.

"The terms 'psychopathic' and 'psychopathy' are used to designate a large group of characteristics or conditions which lie in the broad zone between mental health and mental illness (psychosis)" (footnote, p. 9). Sixteen psychopathic types (the nervous, anxious, sensitive, compulsive, excitable, hyperthymic, depressive, moody, affectively cold, weak willed, impulsive, sexually perverse, hysterical, fantastic, cranks, and eccentric) are described and analyzed in terms of three "levels" of the personality, which are impulse, temperament, and character. "Impulse is an animal, vital urge toward satisfaction of (biological) need" (p. 73). Temperament varies with the fundamental and life mood of the individual, with his excitability and affectivity, and with the tempo, vigor, and form of his expression (p. 172). "Character regulates the interplay between the total personality and the environment" (p. 49), and varies with the attitude of the personality toward the ego and toward the environment and with the interaction of these two attitudes (p. 244). Upon the balance of these three interacting levels depends the degree of concordance or discordance of the personality (p. 52).

The author rejects the pan-sexualism of Freud but accepts as valid the concepts of the unconscious, repression, and the complex. He agrees with Adler in emphasizing the importance of *purpose* or *striving* but criticizes him for the failure to recognize the rôle of *Anlagen,* or genetic predispositions, as causal factors in psychopathic manifestations. Stern's conception of the psychophysical neutrality of the individual is accepted. The treatment of psychopathy is discussed only in the last paragraph of the book, from which the following sentence is quoted: "We are still unable and will perhaps always remain unable to change the constitutional structure of psychopathic personalities, but we can educate many of them to understand themselves, to come to terms with themselves, and to set themselves genuine goals which they may reach in spite of their psychopathic peculiarities."

The volume contains an author index but no subject index. The bibliography of twenty pages contains one French title (Janet) and one English title (a translation of a German work); the remaining titles are of works in German. The writers most frequently referred to, in addition to those already mentioned, are Birnbaum, H. Hoffmann, Kraepelin, Kretschmer, Kronfeld, and Kurt Schneider. The bulk of the material in the volume is evolved by the "arm-chair" method on the basis of the author's clinical experience. The psychologist seeking ready help in his clinical

practice is not likely to find it here. If, however, his interest lies in the theory of personality, and if he is not terrified by an abstruse style and a multitude of new technical terms, his labors will be rewarded by a novel insight into the structure of personality.

New Jersey Reformatory, Annandale D. T. GRIFFIN

Contributi del Laboratorio di Psicologia. By A. GALLI, A. GEMELLI and others. Milan, Publ. Catholic Univ. Sacred Heart, 1931. Pp. 572.

The sixth volume in series six (Biological Sciences) of the publications of the Catholic University of the Sacred Heart (Milan) contains 19 items. Spatial limitations preclude more than a brief statement concerning some of the more important.

A. Galli, The perception of forms in peripheral vision. Perimetric method, Galli found, indicates 5 phases in perceptual reaction: (1) vague existence of something in visual field; (2) some form elements; (3) a figure formed of non-uniform parts, but without significance; (4) precise parted figure with meaning; (5) naming the form.

A. Galli, Concerning the perception of apparent motion produced by different sensory stimuli. Apparent movement studied with variation of number and type of stimuli does not, the author concludes, exclusively support *Gestalt* theory.

A. Galli, Contribution to the study of perception of changes in brightness and illumination. Differential threshold for the perception of both brightness and illumination is increased, this study shows, with the increased speed of stimulus presentation.

A. Gemelli, Researches on the diagnosis of motor ability. Gemelli concludes that the curve of apprenticeship is a more helpful index than aptitude tests.

A. Gemelli and G. Pastori, On the reëducation of decerebrate animals. The authors found that decerebrate fowls are capable of reëducation.

The remaining contributions were by A. Galli and A. Zama, Researches on the perception of plane geometric configurations masked in whole or part by other configurations; A. Gemelli, Emotions and sentiments; A. Gemelli, On the nature and genesis of character; A. Galli, Essay on the psychological analysis of the act of faith in Saint Augustine; A. Galli and L. Nechi, Researches on unstable children; A. Galli, Contributions to the study of moral judgments of normal and abnormal children; A. Gemelli, Current psychotechnical problems in national industry; A. Gemelli, On psychotechnic activity in the laboratory; A. Gemelli, On the nature of manual ability; A. Gemelli, Experimental researches on the forms of voluntary movement; A. Gemelli and A. Galli, On the adaptability of human action to machine action; A. Gemelli, On the value of simple reaction-time with respect to its applications to personal selection; A. Gemelli, Problems of experimental psychology in the study of physical exercise; A. Gemelli, On the selection of aviation pilots.

Indiana University J. R. KANTOR

Le sens de la direction et l'orientation lointaine chez l'homme. By PIERRE JACCARD. Preface by J. Larguier des Bancels. Paris, Bibliothèque Scientifique, Payot, 1932. Pp. 354.

The author of this book on the sense of direction and remote orientation in men is not satisfied to give us a mere statement of this problem as it stands today, but he presents a rather exhaustive account of the way the relevant phenomena have been

regarded in the annals of human thought. On this account, and because of the critical scientific attitude of the author, the book is of significance.

He does not hesitate to bring into the discussion mention of Noah's dove returning to the Ark, and other legendary incidents which indicate early views. Considerable space is given to the legendary, the near scientific, and the scientific inquiry into the behavior of animals as regards their sense of direction and orientation at a distance as well as to that of man. The latter is, of course, the main problem of the treatise. The author notes the efforts of the erudite mind to extricate itself from the anthropomorphic and fanciful interpretation of natural and psychological phenomena. One notes the author's severe criticism of Darwin for disregarding the now esteemed 'law of parsimony' with the implications of animistic meanings, and of even more recent interpretations such as are found in Jacques Loeb's mechanistic conception—for example in his "tropisms." Those who still hold to 'instinct' will find little comfort here, and such speculative writers who are still willing to base their conclusions on reports from travelers on the sense of direction and orientation of the *uncivilized* will find that such peoples when placed under controlled conditions do not possess any such natural sense of direction.

The book is to be regarded as a negative reaction to Levy-Bruhl's theories as they were accepted in France. For the 'mysterious sense of direction' or capacity for distant orientation in either animals or man, the author substitutes as a working principle the guidance by landmarks and other stimuli at hand of all such behavior. While further experimentation is still needed, doubtless this principle will eventually be established. It is noted that American psychologists have not fallen into the pitfalls of anthropomorphic interpretations of such behavior and of other instinctive conduct.

University of Denver THOMAS R. GARTH

Das erlebende Ich und sein Dasein. By OTTO JANSSEN. Berlin and Leipzig, Walter de Gruyter & Co., 1932. Pp. viii, 247.

This book belongs to that German school of philosophical thought the development of which is marked by the names of Husserl, Scheler, and Heidegger. Its phenomenological method and its problems are *not* those of empirical psychology and cannot be represented adequately without a considerable discussion of the philosophical implications. The book can be recommended only to the philosophically interested and trained reader.

The first part discusses the 'living and experiencing ego.' Ego is shown not to be subject to any definition; its chief characteristic is that of being a 'who' and not a 'what.' Emotions, wishes, attitudes in which the ego 'lives' constitute the sphere of the 'ego-belonging' (das Ichzugehörige) as opposed to the 'ego-strange' (das Ichfremde): colors, shapes, etc. However, pathological experience shows that the intrinsic bond between the 'ego' and the 'ego-belonging' can be loosened (depersonalization), and even the apparently impossible duplication of the ego in its own field is achieved in some cases of splitting of personality. In the second part the relation of ego to being and consciousness is discussed and relational theory of consciousness is sharply criticized.

Of interest is the author's evaluation of the phenomenological method. Throughout the whole book he argues against the logistic interpretation of the phenomenological findings which ascribes to them apodictic validity. He shows repeatedly that relationships apparently constitutive for the ego (*e.g.* its non-spatial character, its

unchangeability, its uniqueness in its field) can prove empirically non-valid under special conditions, and are therefore only probable, not necessary. This, as the author himself points out, stresses the empiricism of the phenomenological method, but at the same time the acknowledgment of a discrepancy between phenomenological and empirical findings leads to the problem of validity and limitations of the phenomenological method. It would seem to one that this problem, though seen clearly by the author, is not solved adequately by the introduction of the concept of 'Geltungswechsel:" a mysterious, logically unexplainable, non-temporal change of an intrinsic relationship, which for example makes possible the entering of the essentially non-spatial ego into the sphere of the spatial.

Worcester State Hospital E. HANFMANN

Man as Psychology Sees Him. By EDWARD S. ROBINSON. New York, Macmillan Co., 1932. Pp. vii, 376.

In this book Robinson attempts to present some of the experimental findings of modern psychology together with a general point of view toward man and his nature, in such a way that they can be understood by the above-average reader who is not technically trained in psychology. In the reviewer's opinion he has succeeded admirably in this endeavor.

The book is divided into two parts, the first dealing with man and his nature from the modern psychological point of view, the second part discusses psychology as a science.

In the first part, the topics of motivation, heredity, learning, thought, personality, and the mind-body problem are dealt with. Many psychologists will find the last topic a little out of place but it is adequately treated in a manner comprehensible to most of those who will read the book. The reviewer feels that the first chapter, that on motivation is the weakest, drawing rather heavily as it does on doubtful psychoanalytic concepts and explanations. In justice to the author it should be said that such concepts are presented tentatively, conservatively, and without explicit endorsement. It is doubtful, however, that uncritical readers will recognize this. The chapter on learning, although the most technical in the book, is perhaps the most adequate.

The point of view presented in part two is eclectic—the agreements, rather than the differences among the schools and men in psychology, being stressed. The present limitations of psychological information and doctrine are pointed out.

For a sane, conservative, authoritative book on psychology for the general reader the reviewer knows of none better.

University of North Dakota C. W. TELFORD

Genetic Psychology. By A. R. GILLILAND. New York, Ronald Press Co., 1933. Pp. xii, 351.

The evolution and the development of structures and functions are traced through 20 chapters, the subjects of which are suggestive of the contents. This work on genetic psychology is presented by the author as, "with one exception, the only text of recent times, which reflects the evolutionary or genetic approach to psychology." Chapters I and II are introductory in character. The first states the essential problem of a genetic psychology and the second considers the general characteristics of life and the theories as to its origin. The phylogenetic order from the protozoa to the

human inhabitants of the bronze age is unfolded clearly yet simply through the four succeeding chapters.

The treatment of inheritance in Chapter VI is somewhat more technical than is commonly found in current texts in child psychology and is yet too condensed to be conducive to the greatest clearness. The chapter on prenatal development deals almost exclusively with structure but is appropriately and copiously illustrated. The chapters The Child at Birth, and Physical Growth and Skill, are too condensed to be of any great value even to elementary students, a criticism which holds also for the chapters on music and art, adolescence, the growth of personality, the growth of character, and disease and health. Students in general psychology, child psychology, or educational psychology will find more adequate treatment of these subjects in the current texts of these respective fields. The chapters on play, intelligence, perception, language, memory and thinking, and the emotions are somewhat richer in content and compare favorably with other chapters in contemporary texts.

The author suggests that the book may be employed as a textbook where the approach is biological and that it is especially suitable for collateral reading. The reviewer is more in accord with the second of these suggestions, and believes that the first seven chapters are more valuable for this purpose than the subsequent ones. It is essentially an introduction to psychology through the genetic approach. It would be quite inadequate as a textbook for students who have already had a course in elementary psychology. The lay reader would find it interesting. Each chapter is followed by a short list of selected references.

Iowa State College THOMAS F. VANCE

Integrative Psychology: A Study of Unit Response. By WILLIAM M. MARSTON, C. DALY KING, and ELIZABETH H. MARSTON. New York, Harcourt, Brace & Co., 1931. Pp. xvi, 558.

This book illustrates the folly of trying to write a systematic psychology with all of the impedimenta of a new vocabulary, new definitions and a "unified idea" under the guise of a textbook for beginners. Among the systematic positions adopted, we may name the following: (1) psychology is a science of behavior where behavior is characterized by unique integrative activities; (2) such behavior is to be found only among living creatures possessing a synaptic nervous system, *i.e.* only among the higher animals; (3) the real subject matter of psychology is not behavior at all but the consciousness that is correlated with the integrative action of the organism; (4) the integrative mechanisms in human beings are spontaneously operating devices which free the individual from environmental control and from the taint of materialism; (5) fundamentally, the unit responses of a unit organism are responses of opposition or of alliance. These may be made in the direction of greater or of lesser activity and thus the authors come to four fundamental types of action, viz., dominance, compliance, submission, and inducement.

A considerable amount of the current experimental literature is woven back and forth across this systematic background. One does not get the impression, however, that the systematic ideas have been forged out of the facts. On the contrary, the authors have felt keenly the confusion wrought in the minds of beginners by so many systems of psychology and they have, therefore, solved a teaching problem by inventing another system. It seems doubtful whether this method of writing textbooks

can serve either the task of teaching elementary psychology to beginners or the task of fabricating the systematic foundations of a science.

University of Illinois COLEMAN R. GRIFFITH

The Nature of Human Conflicts, or Emotion, Conflict and Will: An Objective Study of Disorganization and Control of Human Behavior. By ALEXANDER R. LURIA. Tr. from the Russian and edited slightly by W. Horsley Gantt. Preface by the translator and foreword by Adolf Meyer. New York, Liveright Inc., 1932. Pp. xvii, 431, 133 figs.

This book may appeal to persons interested in neurodynamics and as an example of Russian scientific research in psychology. It is an objective study of the motor responses which accompany reactions to controlled word association. Graphic records of these responses are compared and significant variations are linked with inner conflict. Luria assumes that these motor responses are of two main sorts: (1) those produced when the personality suffers no conflict; and (2) when there is conflict. The former responses are immediate and smooth while the latter are both delayed and irregular. He explains the disturbed motor response as due to a disorganization of culturally instilled psychological mechanisms and a reversion to simpler and lower levels of reaction. That speech does not function smoothly when there is conflict is due to the same causes that make motor response atypical, *i.e.* speech depends upon the same psychological mechanisms of a higher sort instilled or modified by cultural experiences.

His cases include children as well as adults, the abnormal as well as the normal, and normal adults tested under both usual and unusual conditions.

His final chapter on the "Control of Behavior" is theoretic but convincing. For instance, his view that the use of vocal or subvocal speech is an effective autostimulus for desired action finds ample support in everyday life since speech frequently accompanies the performance of difficult motor operations, as the golf novice will testify. That this speech is a cause of properly timed coördinations is probable but not so obvious.

The book as a whole is scholarly and valuable but it is so difficult to read as to make it improbable that it will have wide use.

Trinity College, Hartford, Conn. R. B. W. HUTT

An Experimental Manual in Psychology. By JOHN F. DASHIELL. New York, Houghton Mifflin Co., 1931. Pp. ix, 161.

This manual is designed to be used in conjunction with the author's textbook, the *Fundamentals of Objective Psychology.* It is admirably suited for this purpose, since every important topic in the text is paralleled by a concrete experiment. The objective approach is emphasized, though this does not preclude experiments in sensory, perceptual, and higher thought processes. Topics of current interest receive due emphasis, such as motivation, emotion, personality, social influences, the conditioned reflex, and fatigue.

Each experiment is logically arranged with problem, materials, procedure, results, questions for discussion, and pertinent references clearly outlined.

Illustrative cuts of apparatus accompany most of the topics, which supplement the wealth of photographic illustrations in the author's text. An appendix contains copious notes for instructors, including a list of psychological supply houses and instructions about apparatus.

Although fitted to beginning students, the manual is extensive enough for profitable use by more advanced students, and the range of topics is sufficiently broad to be useful as an accompaniment to almost any introductory textbook in psychology.

University of Chicago A. G. BILLS

Psychology of Sex. By HAVELOCK ELLIS. New York, Ray Long and Richard R. Smith, Inc., 1933. Pp. xii, 377.

The author of this book is also responsible for the classic five-volume study on the subject of psychology of sex. The present monograph is largely a modernized and practical abstract of the larger work, and this process of modernization has made it much more valuable for the clinician, particularly for the physician who must discuss sex problems with his patients. The general tone of the book is descriptive. While the actual scientific validity of the information which it contains is not impaired by this fact, investigations are discussed from the point of view of results and their significance rather than from that of experimental procedures. In this respect it resembles the work of Freud which Ellis uses in a simple and understandable fashion, neither accepting it completely nor rejecting it. The usual topics to be found in a book of this nature are considered, such as the biology of sex, description of sexual impulse in youth, sexual deviation and homosexuality. There are also chapters on marriage and the art of love, which topics are dealt with in a generalized but sane fashion. Case histories are of necessity lacking, but the bibliographies on each topic are excellent. A teacher of sex education or a gynecologist should be able to find considerable use for this book, but the researcher into sexual psychology will find Ellis' older work, supplemented by the references given, more valuable.

Institute for Juvenile Research LOWELL S. SELLING

Experimental Social Psychology. By GARDNER MURPHY and LOIS BARCLAY MURPHY. New York, Harper & Bros., 1931. Pp. ix, 709.

If there is any truth in the statement that men have an almost unlimited capacity for resisting the application of knowledge to their own affairs, a part of the truth must derive from the fact that the amount of reliable knowledge has been woefully small. The book now before us plainly says that some of this knowledge is at hand. It says, also, that methods for getting more knowledge are gradually emerging. There is, therefore, a great difference in the language and in the mood of the present book as compared with earlier works on the subject. It is obvious, of course, from such chapter headings as "The Biology of Motives," "Nature and Nurture in the Causation of Individual Differences," "The Learning Process in Social Situations," and "The Development of Social Behavior in Early Childhood," that *Experimental Social Psychology* is an amalgam of several other psychologies; but this fact is, perhaps, irrelevant when we consider the great advantage that comes from having the experimental material on problems of a social sort brought together in a single volume. At times, the chapters read as though a collection of abstracts had been pasted together; but even this can be pardoned, pending the day when the same authors, perhaps, can review the field more systematically and critically. The book is richly documented and it shows abundantly that the social implications of psychological facts can have a reputable meaning.

University of Illinois COLEMAN R. GRIFFITH

Religious Behavior. By DAVID M. TROUT. New York, Macmillan Co., 1931. Pp. xiv, 528.

The author of this book has attempted to describe in objective terms a group of phenomena which are, by tacit acceptance, spiritual, and therefore beyond the ken of behavioristic analysis. On the other hand the point of view is not behavioristic in the systemic or mechanistic sense. The approach is avowedly organismic, which may account for the fact that it is sometimes a bit involved.

To start with there is a new definition of religion which is borrowed from Paul's *Epistle to the Corinthians;* namely, that the abiding religious realities are hope, faith, and love. These are treated as organic and purposive attitudes, rather than subjective entities. Goal-seeking becomes, therefore, the test of religiosity. The definition is later expanded to imply that telic behavior must also be loving behavior in order to be religious. The goals must be social rather than selfish. Being religious is a matter of adjustment to the total organismic pattern of life.

In its emphasis on a sort of mystical rapport with the race, it leaves out fear, hate, and self-service which have played a part in all religions.

The treatment is original throughout. A survey of researches and a suggested list of problems is included. There is an excellent bibliography.

University of Colorado THOMAS H. HOWELLS

Die gerichtspsychologische Begutachtung von Autounfällen und die Eignung zum Chauffeur. By KARL MARBE. Leipzig, Hirschfeld, 1932. Pp. 64.

The author discusses the actions performed by locomotive engineers, truck drivers, and automobilists in a brief, but clear and exact manner. He recites his experiences as expert in court. The American psychologist will find little in this book that is new to him. Perhaps the author's contention (among many other suggestions) that the wording of laws and orders must be free from inducing that which is actually wrong, is worthy of experimental study in America. When a locomotive engineer, *e.g.* is ordered "to enter a siding in spite of the stop signal and then to proceed further," can he be blamed for overruling also that stop signal which he sees at the end of the siding and for thus causing a wreck? Or, when a motorist in a certain city lawfully crosses a street where a sign prohibits turning to the right, and then lawfully crosses a street where a sign prohibits turning to the left, and then approaches a crossing where the conspicuous sign reads "turn right or left" without mentioning a prohibition, can he be blamed for crossing straight ahead and overlooking an inconspicuous sign 25 ft. sidewise which reads "no forward passage"?

University of Missouri MAX F. MEYER

Psychodynamik des primitiven Denkens. By F. M. FELLER. Leipzig and Vienna, Franz Deuticke, 1933. Pp. vi, 57.

In this very short book the author points out his solution of some of the ethnological problems, such as totemism, the taboo of dead bodies, exogamy, and other primitive ideas. His method is the study of the findings of early ethnologists rather than the analysis of any new data of his own deduction. His expression and interpretation are on a basis developed by combining Wundt's laws of association and deep psychoanalysis. It is his idea that primitive people do not differ from those of the present day in that they do not advance so far in development, but rather that they develop along different channels and have the capacity for the same development.

Some of his interpretations seem a bit far fetched but since they are derived by means of a type of psychological study which is not yet entirely approved by psychologists and since he comes to his conclusions largely through logical methods, too much cannot be expected. Such a book is undoubtedly of more interest to ethnologists than to psychologists.

Institute of Juvenile Research LOWELL S. SELLING
Chicago

The Psychology of Study. By C. A. MACE. New York, R. M. McBride & Co., 1932. Pp. viii, 96.

Six essays give a common sense summary of thoughts upon study. Mental functions are dealt with in a severely practical way. Man, it is assumed, "is an instrument for responding to stimulation in ways which suit his purpose." The discussion centers about perception, memorization, originality and thinking, concentration and the will to work.

A fundamental maxim of the writer emphasizes letting questions come, getting them clear, following their lead, and then asking one's self another question. The importance of the organization of habit and of the affective disposition is emphasized. Study requires the will to work and the organization of external conditions. Except for extreme cases, individual differences boil down to a difference in speed. References are to F. Bacon, Chesterton, Coué, Descartes, Ebbinghaus, Freud, Roger Fry, Henry and William James, Locke, McDougall, Newton, Nietzche, and Torricelli; experimental workers receiving relatively little recognition.

In an interesting and pleasing style the author presents what is commonly accepted as useful information about studying; some of the statements will be questioned by psychologists, but in general the book can be recommended as giving useful suggestions to the general reader.

University of Georgia A. S. EDWARDS

Hygienische und gewerbetoxikologische Untersuchungsmethoden. By EUGEN LEDERER. Abderhalden's Handbuch der biologischen Arbeitsmethoden, IV; Teil 16. Berlin and Vienna, Urban & Schwarzenberg, 1932. Pp. 519-932.

While there is little in these 400 pages which can be said directly to be psychology, there is much technological information which occasionally will be welcome especially to him who is working in the field of applied psychology. For instance, climatic conditions, temperature, moisture, air movement, clothing are influences which sometimes concern the psychologist. Likewise illumination by daylight or artificial light, seating arrangements at various kinds of work, noises which disturb the worker, vibrations so strong that they are a danger to health. A greater number of measuring instruments are described, chiefly of German design and manufacture. Specially emphasized among them are those for determining the presence of poisonous gases in the air in great dilution.

University of Missouri MAX F. MEYER

The Logic of Science. By H. R. SMART. New York, D. Appleton & Co., 1931. Pp. x, 237.

This is a fairly readable introduction to the thesis that the peculiar nature of the relation between the object and the subject in scientific knowledge is a logical rather

than a physical or mental relationship. That is, the function of a logic of science is to search out those abiding features in the content and form of scientific knowledge which serve to identify the several branches of science and give unique standing to the whole of experimental inquiry. Throughout a series of eight chapters in which the author is obviously more at home when speaking of the mathematical and the physical sciences than of the biological and psychological sciences, the "abiding features" of scientific method are identified. These features touch psychology at two points, viz., in the definition of subject matter and in the relations between psychology and logic. The author suggests that psychology take mind as the biologist takes life, viz., in a functional rather than in an existential way. Since psychology studies the origin and development of functions out of which logical relationships can spring, the relations between logic and psychology are of a mutually dependent order.

University of Illinois COLEMAN R. GRIFFITH

Workbook in Elementary Psychology. By A. E. BROWN. New York, Macmillan Co., 1932. Pp. iii, 182.

Instructors who give large classes in semester or quarter courses will be interested in this teaching device. The workbook is based chiefly on the revised edition of Gates' *Elementary Psychology,* but it contains optional references to a half dozen other texts. A provision for conference groups within the class as a whole overcomes the objection that this method of instruction is mechanical and stereotyped. A sufficient variety of material is offered to prevent the student from becoming bored in his work. In addition to the forty-one study units, four review lessons are included. Each study unit contains (1) directions for study; (2) additional notes, which are valuable to beginning students; and (3) an assortment of study exercises or test questions of the yes-no, matching, completion, enumeration, analogy, and discussion type. The sheets are perforated and punched so that they may be removed, turned in to the instructor, and filed in the standard 8½ x 11 in. notebook.

Southwestern College W. R. ATKINSON

Endocrine Medicine. Vol. IV. By WILLIAM ENGELBACH. Foreword by Lewellys F. Barker. Springfield, Ill., and Baltimore, Md., Charles C Thomas, 1932. Pp. 117.

This volume contains 49 pages of bibliography classified for the several chapters in the first three volumes, an index of names of 14 pages, and a 47-page index of subjects. With these aids the reader is able to locate readily any subject dealt with in the first three volumes, as well as the original sources drawn upon by the author. The three earlier volumes of the series (reviewed in this JOURNAL, 46, 1934, 169) deal respectively with "general considerations," the infantile and the juvenile endocrinopathies, and the adolescent and the adult endocrinopathies. The volume will be useful to psychologists interested in the problems involved in this field.

J. P.

Methoden des Tierversuches in der Arbeitsmedizin. By EBERHARD GROSS and HERMANN HEBESTREIT. Abderhalden's Handbuch der biologischen Arbeitsmethoden, IV, Teil 16. Berlin and Vienna, Urban & Schwarzenberg, 1932. Pp. 933-1025.

This is a detailed discussion of the methods used in experimental studies of the effect on animals of injurious substances when acting on the lungs, the digestive tract, and the resorbing skin, or when irritating the skin. Much technological information.

University of Missouri MAX F. MEYER

The Evolution of Human Behavior. By C. J. WARDEN. New York, Macmillan Co., 1932. Pp. ix, 248.

In this book the author offers a treatment that is "as non-technical as could well be without sacrificing something of scientific accuracy" of the evolution of human behavior, rather than the evolution of human bodily structure.

In the first part of the book, after a clear, brief treatment of the organism-environment relationship both in psychology and in evolution, a review is presented of the traditional biological problems of evolution. The next chapter is devoted to a consideration of the natural kinship of man to infra-human animals. In this chapter emphasis is placed upon morphological development in the phylogenetic series according to the manner of the classical evolutionists. A number of diagrams are here reproduced from strictly biological works. One of these diagrams, taken from Tilney's "The Brain from Ape to Man," showing the developing "primate horizons" with precise indications of association areas and the like, may possibly be misleading when presented as divorced from the complete setting of its original text.

The next sections of the work deal with the progress of the anthropoid into the true human individual. Obviously much of this treatment must be speculative. In this part of the book such problems as the morphological kinship of ape and man and the phylogeny of language are briefly discussed. In the latter part of this section much reliance is placed upon the sort of evidence presented by Osborne in his book, "Man Rises to Parnassus," and is subject to the same favorable or unfavorable criticisms that have been given to that book. Superimposed upon this background, a brief review is presented of man since the upper paleolithic time. Concerning this development Dr. Warden writes: "The appearance of this new species marked the last great advance in structural evolution and ushered in the age of modern man. Homo sapiens possessed a larger and better balanced brain than any of his predecessors. He was distinguished also by numerous refinements in bodily form and by superior manipulative skill and language capacity. In time, the natural increase in numbers in the homeland forced the species to spread over the earth, and to become diversified into the three great racial stocks—negroid, mongoloid, and caucasoid."

The last chapter of the book is devoted to an evaluation of present trends in evolution. Here the position is taken that the urbanization and degeneration of the "great race" does not mean the end of human culture, "so long as other races are left behind which have the physical strength and mental caliber to carry on."

As E. G. Conklin pointed out a number of years ago, scientific biology during the present century has largely turned from the speculations of phylogeny to the experimental study of ontogeny. A similar shift in emphasis can be seen in scientific genetic psychology. This shift does not mean, however, that problems of racial development such as those treated in this book are uninteresting or that they should be neglected. It merely points to the fact that they are peculiarly difficult to deal with from the scientific standpoint, as they are not even potentially open to experimentation. This situation probably explains the fact that in the present book there are not many references to the work of experimental psychologists, save such material as is given in a review of Wundt's speculations in regard to language and in a few similar excerpts. In some respects it is, however, surprising that the

author has considered reference to the experimental studies of anthropoid behavior inappropriate to certain parts of the argument.

From the standpoint of the reviewer, however, the major shortcoming of the treatment is the almost complete neglect of the ontogenetic evolutionary problem. This omission has led the author to avoid any reference to the processes by which the alleged developments of the race become effective in the individual. It might not have been without value, for example, to consider the evidence of modern ontogenetic studies in evaluating the phylogenetic theory of the behavior of children who walk on all fours. A detailed consideration of ontogeny might well have taken the present volume beyond the bounds set by the demands of a popular book. But at all times especial care must, it seems, be taken by the necessarily speculative student of phylogeny not to attribute to the essentially hypothetical racial environment phenomena which may experimentally be shown to result from the interaction of the environment and the developing organism in ontogeny.

An excellent brief list of references is given at the close of the book.

Brown University LEONARD CARMICHAEL

Crime, Law, and Social Science. By JEROME MICHAEL and MORTIMER J. ADLER. New York, Harcourt, Brace & Co., 1933. Pp. xxix, 440.

Here is a penetrating logical analysis of the relation of the social sciences to the practical problems of criminal justice, a revision of a survey originally made under the auspices of the School of Law of Columbia University for the Bureau of Social Hygiene. The authors, a legal scholar and a philosopher, have formulated the theoretical problems of the criminal law, examined and evaluated the research in this field, and summarized their conclusions and recommendations in the form of a plan for an institute of criminology and criminal justice.

The analysis is thorough, the logic incisive and compelling, the conclusions positive and devastating. The argument running through the book, and oft repeated, is that in the present state of our knowledge no empirical science of criminology is possible, for such a science must depend on an empirical science of psychology and of sociology, which as such, are non-existent. Psychology, it seems, is suffering from dull logical tools and an immature epistemology. The failure of psychology to measure up to the exacting requirements of an empirical science as formulated and defined by the authors, is attributed to the lack of definition of subject matter and a misconception of the nature of the scientific method. Most of the material presented by psychologists, it is claimed, is physiological rather than psychological. Furthermore, psychologists in their attempt to get away from a simple rational science which is no longer respectable psychology, have swung to the other extreme. With "an exclusive emphasis upon observation to the total neglect of the abstractions of analysis" they have thrown the subject over to a "raw empiricism." Data yielded by psychological investigations of criminal behavior are both invalid and insignificant because the concepts prerequisite to observation are not clearly defined and the statistical descriptions utterly lack direction by theoretical analysis. These contributions therefore amount to little more than descriptive knowledge.

An extensive, if not intensive, survey of the many research studies in the causation, treatment and prevention of crime lead the authors to the conclusion that the indispensable requirements of scientific procedure and method have not been met. They have failed to give us any knowledge whatever of the etiology of

crime, for they have failed to formulate the causal problem in terms of the dependent and independent variables; nor can they tell us whether criminals can or cannot be differentiated from non-criminals in terms of any set of factors. Though the authors distinguish between the subject matters of psychology and sociology they have a blanket condemnation of research studies contributed by both. Many of the studies cited are not, of course, the work of psychologists.

On the constructive side they propose plans for an institute of criminology and criminal justice to attempt a solution of the two basic problems, the theoretical problem of etiology of criminal behavior and the practical problem of control. They recommend that the kind of research that is now being done be discontinued. It is significant that in the criminological division of the proposed institute, psychologists and sociologists would be replaced by logicians, mathematicians, statisticians, and physicists, the latter for their knowledge of the basic concepts of empirical science. It is the contention of the authors that the psychologists and the sociologists, lacking broad philosophical orientation, adequate knowledge of the methodology of science and the instruments of logic and mathematics, "have shown themselves utterly incompetent to construct a science of criminology." An exception is made of the special field of psychometry where an empirical science is developing.

A weakness of the book is its self-conscious attempt on theoretical grounds to place all psychology outside of the closely circumscribed "empirical sciences" without adequate consideration of psychological contributions other than criminological. This thought-stimulating book is, however, a challenge to psychologists, sociologists, and other students of criminal behavior. To such it is a valuable and, in the reviewer's opinion, a very important contribution.

Dartmouth College EDWIN POWERS

Revelation in Mormonism. By GEORGE B. ARBAUGH. Chicago, Univ. Chicago Press, 1932. Pp. x, 252.

This book constitutes an excellent case-study of the development of a sacerdotal religion. Of all the major anthropomorphic religions Mormonism is unique in that it is more modern, that the sources are observable, and that more critical attention has been given to them. In spite of the fact that the background of Mormonism has been considerably blurred and distorted because of the bias of early proponents or opponents, the mass of evidence seems adequate to characterize it.

The author presents a very intimate account of the various expediencies of the promoters of an organized cult. The first part deals with the craze of a century ago in rural New England for the discovery of buried treasure and lost manuscripts of ancient times. This craze infected Sidney Rigdon and Joseph Smith and culminated in the Book of Mormon and several other books and pamphlets, and finally in the restoration of an ancient but lost priesthood and church organization. The second part deals with adaptive changes by which revelations became familiar and mundane, and were used by Joseph Smith to obtain for himself a monopoly in seership and social control. The third part presents a period of competitive and contradictory revelations, in affirmation or rebuttal of various claims of succession to the headship of the church after the death of Joseph Smith. A number of the resultant sects are yet in existence, but revelation, in the old sense, has been tacitly abolished by the larger groups and the heavens are again silent.

The mystic element has never been prominent in Mormon revelation. The appeal

has always been in terms of economic and familial advantage, institutional sanction, and theological phantasy, rather than in ecstatic rapport with the Divine. It is notable that Mormonism has never inspired any important poetry, art, or music.

The apparent service of this study is chiefly that it does a thorough job of assembling the available data. The author does not attempt a great deal in the way of either a psychological or a sociological interpretation of these data. He holds that the revelations were altogether the product of deliberate fraud, rather than of split personality as advocated by I. W. Riley. The obvious opportunism in them is cited in support of this view.

There is virtue of course in calling a spade a spade, but on the other hand one can be too dogmatic about his categories, especially in regard to religious and spiritistic phenomena. Deliberate fraud, self-deception, hallucination, and social leadership are, in religion, such a part and parcel of each other that they are seldom seen in pure form. A comparative interpretation is always more truthful. This fact is demonstrated by the author's own testimony that the defrauded, gullible ones became in their turn the defrauders. In most religions there seems to be a queer paradox of coexisting deceit and sincerity.

There is an abundance of references to sources, and a valuable bibliography is appended.

University of Colorado THOMAS H. HOWELLS

The Gestalt Theory and the Problem of Configuration. By BRUNO PETERMANN. Tr. by Meyer Fortes. New York, Harcourt, Brace & Co., 1932. Pp. xi, 344.

This treatise on the Gestalt theory is unique in its presentation in that the author does not overwhelm the reader by shoving the whole system before him at once. By a careful analysis of publications on the subject Petermann introduces one concept after another in their genetic order. By this method he is able to take into account the shifting of standpoints within the literature. Furthermore, it enables him to attribute the various concepts to individuals rather than to the system as such. Finally, Petermann's genetic method enables him to present systematic concepts in their inextricable relation to experimental findings.

Within the last score of years we have heard much of the "new" Gestalt Psychology which is purported to have had its origin in Wertheimer's investigation, *Über das Sehen von Bewegung,* in 1912. Petermann shows that the principle characteristics of the "total situation" of behavior, which seem to constitute the groundwork of the configurational theory, are traced in their progressive development from Von Ehrenfels through Cornelius and Lipps. Other contributions to this concept are Benussi's "Production" theory, and G. E. Müller's "Coherence" theory.

After giving us a clear statement of the working principles of the Gestalt theory in Book I, Petermann summarizes the systematic field of the theory as follows: (1) The theory is not confined to psychology. Its essential principles are applicable to biology and physics as well. (2) Psychological Gestalten are essentially physical prototypes. (3) In biology the theory attempts to resolve the controversy between vitalism and mechanism. (4) In psychology, it undertakes to derive "the entire range of psychical phenomena from one unitary and determinate basal doctrine, in the form of a unitary theory."

In Part 2 of Book II there appears a review of the literature on figural structures upon which the Gestalt theory has relied for empirical verification of its proposed

laws. Special emphasis is placed upon Wertheimer's "Gestalt factors," and Köhler's interpretation of these. It is concluded that Wertheimer's description gives an accurate account of the spatial collocation which takes place in the building up of figural unity. However, Wertheimer's description "by no means brings analysis to an end." Petermann introduces here his own theory of "cognizance in the mode of apprehension." He believes that in addition to the factors taken to be responsible for contrast, reversible perspective and other figural phenomena, attentive characteristics come into play. In general, Petermann takes the attitude that in their zealous intent to construct a psychological system Köhler and Koffka have lost sight of the "subjective conditionalities of our Gestalt experiences, which are under certain circumstances so conspicuously exhibited." It should, however, be pointed out that the term "attentive apprehension" when inadequately defined, contains no more analytic value than the magic term *Gestalt* and its accessories.

In evaluating the book as a whole we readily admit that it is the clearest statement and the profoundest critical assessment of the configuration theory now available to the current English speaking public. We should add that we are greatly indebted to the translator, Meyer Fortes, who has done a difficult task well.

University of Vermont P. H. EWERT

Principles of Judicial Proof. By JOHN H. WIGMORE. 2nd ed. Boston, Little, Brown & Co., 1931. Pp. xix, 1056.

The first edition of this book (1913) has long been favorably known to those psychologists who are interested in the testimonial process. The book has been particularly useful because it presented fairly a generalization of psychological experimentation in the field, and an evaluation of it by the foremost American legal authority on evidence. Its purpose was to set forth a method whereby each bit of evidence might be evaluated as it comes, the value indicated by a symbol, and the symbol placed in a schema. It was the author's hope that by this method the final evaluation of the testimony as a whole might be made on the basis of the sum of the evaluations as given simultaneously in the schema, instead of the usual method of cumulative evaluation as the testimony is received. The plan, however, as originally stated did not receive the attention either of jurists or of psychologists that it deserved; and in this new edition, which is entirely revised and re-written, the author attempts not only to bring the book up to date but also to place his method in bolder relief. The book is devoted to an analysis of the whole evidential process in the interest of this method, and at every turn the author finds psychological factors like emotion, habit, perception, recollection, imagination, intention, belief, temperament, and the like. Moreover, in addition to an excellent digest of the *Aussage* experiments, the author, in a chapter entitled "Experimental Psychometry," presents in great detail the blood pressure ("Lie-Detector") method of detecting guilt with particular reference to Keeler's work. There is also, in the same chapter, a revised account of the word-association method in which he quotes extensively from Crosland's monograph. The author thus shows his interest in these experiments although he does not think that at the present time they are of great value.

In many ways the book is a challenge to psychology. The author's method of evaluation of testimony itself raises a problem for the psychology of conviction. Frequently, moreover, statements are made which a psychologist may question but

which cannot be answered save by experimentation. On the whole, however, Wigmore's psychology is admirable, coming as it does from one who is outside the field. The book's excellent digests and bibliographies, and its suggestiveness make it invaluable to anyone concerned with the application of psychology to the problems of judicial proof.

H. P. WELD

Cornell University

Meaning and Change of Meaning. By GUSTAV STERN. Göteborg, Wettergren & Kerbers, 1931. Pp. xiii, 456.

This book should prove a godsend to those psychologists who are struggling with problems of language, containing as it does not only a great mass of linguistic material, conveniently arranged and carefully documented, but also a full and thoroughly up-to-date discussion of the problems of verbal meaning. No other existing work approaches it in scope and excellence, and it is improbable that a successful rival will appear for some years to come.

The present-day psychology of language suffers from two outstanding defects. In the first place the psychological study of language presents so many intrinsic difficulties that the laboratory scientist, fearful of embarking on a voyage which might carry him beyond the confines of his neatly furnished work-shop, has usually been led to ignore the more complex semantic problems and limit himself to semi-physiological experiments in phonetics. In the second place, students of linguistics proper who have adopted a physiological approach have all too frequently failed to realize that psychology has undergone considerable change since the days of Wundt's *Völkerpsychologie.* We have in consequence, with a few notable exceptions, a psychology of language which is either inadequate in scope or archaic in point of view.

One of the merits of Professor Stern's book is that it is open to neither of these charges. He has shirked no problem because of its difficulty, and he displays a knowledge and a grasp of recent psychological literature which should put many a *Fach*-psychologist to shame. The book is presented as a first textbook in semasiology, devoting itself in the first half to a psychological analysis of verbal meaning in terms of linguistic functions, and in the second half to a classification and a detailed study in terms of the same functions of seven fundamental types of sense-change. Stern's psychological point of view is functionalistic, and his approach genetic. His principal linguistic source is the New English Dictionary, and for his psychological material he draws with equal willingness from recent publications in English, French, and German. It is evident that he leans heavily on Head's psychopathological studies, and it is gratifying to find that he attaches due importance to the too-seldom read works of Selz. The reviewer has failed to observe any serious omissions or misinterpretations.

Cornell University R. B. MCLEOD

Some Effects of Incentives: A Study of Individual Differences in Rivalry. By JOSEPH ZUBIN. (Contr. to Educ., No. 532.) New York, Bureau of Publications, Teachers College, Columbia University, 1932. Pp. 60.

This is a well controlled experimental study designed to measure the effect, on certain functions, of motivated individual versus individual rivalry, and to determine

whether the individual differences shown were reliable. The functions involved are (1) addition, and (2) comparison of numbers. The "motivated" individual rivalry was obtained by promising a prize to each pupil who should excel his immediate superior in rank order. The children were not told what the prize would be. In the non-motivated rivalry it was assumed that no incentive was present. The subjects were 217 school children from Grades 6, 7, and 8. The results show an average gain of 5.5 ± .33 examples per min. in the incentive periods over the non-incentive periods; and a smaller coefficient of variation of incentive speeds than of non-incentive speeds. For addition the difference was 1.70 ± .55, and for comparisons it was 1.97 ± .30. The correlation of average gain with age was 0.11 ± .06, and with intelligence 0.13 ± .07.

The study offers little that is new in technique. Since the meaning of a "prize" is not the same to all children, it is, of course, not accurate to assume an incentive of equal force for each child. The narrow range of activities and interests of the group used prevents broad generalizations; this limitation, however, suggests further investigations both above and below the ages and grades studied here.

University of Texas O. B. DOUGLAS

Why We Oppose the Occult. By EMILE CAILLIET. Tr. by George Franklin Cole. Philadelphia, University of Pennsylvania Press, 1931. Pp. v, 200.

"To Professor Edgar A. Singer, Jr., this study, made at the University of Pennsylvania under the auspices of the Adam Seybert Foundation of Intellectual and Moral Philosophy, is respectfully dedicated." Thus, this study is apparently a successor to the famous "Preliminary Report" of the Seybert Commission on spiritistic phenomena in 1887, and supported by funds from the first bequest ($60,000) to be accepted by an American university to provide for research in the field of metapsychics.

The author has brought together anthropological data, some of it obtained during a sojourn of four years in Madagascar, supporting his thesis that the violent opposition to the occult expressed by modern society issues from an atavistic terror of magic and the dead, a horror induced by the association of ideas that has "branded the occult with indelible scars." That this atavistic fear is 'contagious' in civilized society he illustrates by an experience of his own (p. 94). His treatment of the relations of magic to religion and to science, will interest the psychologist with anthropological leanings, his delineation of social forces issuing from magic will interest the student of social psychology who is now engrossed in the measurement of attitudes, and his illustrations of the potency of magic offer data to the student of hypnotism and psychopathology. The student of metapsychics will be interested especially in the introduction, beginning with a quotation from Bergson which sets the problem, and in Part III which contains the chapters on "Spiritualism challenged," "The dilemma of metapsychics," and "A false front." The dilemma is that metapsychics in seeking to divest itself of spiritism and the supernatural, and to confine itself to the abnormal, yields its place in the realm of science to psychopathology. Science, including experimental psychology, does not oppose the occult, but undermines it.

The book offers little to interest the experimental psychologist. It has no index, but contains an appendix of 14 pages of citations and notes.

Stanford University J. E. COOVER

Schule der Abstraktion und der Dialektik: Neue Wege begrifflichen Denkens. By LEO JORDAN. Munich, Ernst Reinhardt, 1932. Pp. 160.

The author attacks the philosophical concepts of abstraction and dialectics from a linguistic point of view. He finds that the development of the meaning of words in children and the various differences in shades of meaning of corresponding words in the Japanese and German languages do not substantiate the philosophical analysis.

Philosophy, for example, ignores the analysis of percepts. When a child uses a word, Jordan holds, this word refers to a characteristic trait which stands out in the percept. All objects having the same trait are referred to by the same class name. Later experience determines whether the first analysis will be satisfactory. A word, therefore, does not have reference to a common element of a class, but rather to any characteristic which happens to stand out in the perceptual experience. The experience of meaning, not the etymology of the word, must be the point of departure in any study of abstraction. Abstraction in the true sense is therefore not the isolation of a common element, but the experience of a relationship; and relationships cannot be perceived.

Dialectic is defined as double abstraction. "Good or bad," for example, contains two abstractions which appear to be opposites, but when used together they form a single new abstraction having a new meaning. Combining abstractions in this manner give us abstractions of a higher order.

Jordan's treatment is of philosophical rather than of psychological interest. It is divided into an analytic and a synthetic part, the method of approach being primarily epistomological.

University of Michigan N. R. F. MAIER

The Dramatic Personality of Jesus. By KNIGHT DUNLAP and ROBERT S. GILL. Baltimore, The Williams & Wilkins Co., 1933. Pp. vi, 186.

All the work of Jung, Rosanoff, Prince, Jaensch, Spranger, and the rest of our modern students of personality is here ignored. All of the scholars who have seriously argued that Jesus was psychopathic are brushed aside with the three words, "not a psychopath." There are good chapters on the racial and social backgrounds, the ethical teachings, the theology of Jesus, and, what is perhaps the best of all, a chapter on the Jesus of the gospels, in which the available evidences from the gospels and gospel sources are well brought together with references given. But such collections of Biblical references are not unique, and, unfortunately, the many others who have worked on these subjects are not mentioned. Several pages are given to the doctrine of the virgin birth, but the contribution of this doctrine to the personality pattern of Jesus, if any, is not made clear. Many traits are itemized and Jesus is said to have been well-adjusted to sex problems; but again these are nowhere brought together and used as indications of the kind of personality Jesus may have presented to his associates. The style indicates familiarity with the literature on the social backgrounds and the various problems concerning Jesus and his teachings, but only the names of Clay and Malinowski are actually mentioned and these appear without specific reference. There is no bibliography, and no index, and there are no footnote references to literature other than the Bible itself.

University of Oregon EDMUND S. CONKLIN